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THE
PHILOSOPHICAL MAGAZINE
AND JOURNAL.

31st JANUARY 1826.

I. *A new Catalogue of Meteoric Stones, Masses of Meteoric Iron, and other Substances, the Fall of which has been made known, down to the present Time.* By E. F. F. CHLADNI.*

§ I. *Introduction.*

IT is my intention to offer, in the present memoir, a complete and rectified catalogue of all the phænomena of this description that have been observed from the earliest ages down to the present time. Since the publication of my work on *Igneous Meteors and the Substances that have fallen from them*†; in which I treated this subject as fully as I was able, new occurrences of the same description have taken place; and these I published, by way of appendices to my work, in vol. lxviii. p. 329, and in vol. lxxi. p. [illegible] of Gilbert's *Annals*.

In the present catalogue, I shall, in order to avoid prolixity, forbear mentioning the sources of my information [illegible] phænomena as are treated of in the above-mentioned [illegible] but I shall cite them if the fact has not [illegible] I shall also omit all those phænomena [illegible] belong to this class (for instance, where hail has been mistaken for the fall of meteoric stones); and where I cannot omit I shall insert them in parentheses, for the purpose of showing that they are extraneous. If uncertain, I shall prefix to them a note of interrogation. Those mentioned in my work are preceded by an asterisk.

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* From Schweigger's *Neues Journal*, B. vi. p. 87. In order to make this catalogue of meteorites (which is the latest that has been drawn up,) as complete as possible, we have inserted notices of a mass of meteoric iron, and the fall of some meteoric stones, which have lately been communicated to the same Journal by Prof. Nœggerath; and we have also appended to it some particulars of the various falls of meteorites that have taken place since Dec. 1822, when the catalogue was first published, as well as of some masses of iron subsequently discovered. Our additions are distinguished by insertion within *brackets*.—EDIT.

† *Ueber Feuer-meteore und über die mit denselben herabgefallenen Massen*, in one vol. 8vo; accompanied by an appendix with 10 lithographic prints by Schreibers, in folio; published at Vienna in 1819.

§ II. *Falls of Meteoric Stones and Masses of Iron.*

A. *Before the Christian Æra.*

Division 1.—*Containing those the time of the fall of which can be indicated with some degree of certainty.*

? 1478 B.C. In Crete, on the Cybeline mountain, the stone considered as the symbol of Cybele, with which Pythagoras was initiated into the mysteries of the *Idæi Dactyli.*

(The narrative in the book of Joshua of stones having fallen from heaven probably alludes to a hail-storm.)

? 1403. Perhaps a mass of iron fell on Mount Ida in Crete.

1200. Stones preserved in the temple at Orchomenos.

? 705 or 704. The Ancyle: most probably a lump of iron somewhat flattened.

654. Stones on the Albanian mountain.

644. In China.

465. A large stone near Ægos-potamos.

Not long before or after. A stone near Thebes.

211. A remarkable fall of a stone near Tong-Kien in China.

During the period of the second Punic war, probably about 206 or 205. Fiery stones.

192. In China.

176. A stone in *agro Crustumino* in the Lake of Mars.

99 or 89. *Lateribus coctis pluit*, probably at Rome.

89. Stones in China.

56 or 52. In Lucania (a district which consisted of part of the present Abruzzo, Apulia, and Calabria), spongy iron. (I believe that I am in possession of a small fragment of this iron, as I shall have occasion to show in sect. iii. B.)

? Perhaps stones, perhaps hail, near Acilla.

38, 29, 22, 19, 12, 9, 6, in the first moon, and 6, in the ninth moon. Stones in China.

Division 2.—*The time of the fall of the following is indeterminable.*

The stone which fell at Pessinus in Phrygia, which was considered as a symbol of the Mother of the Gods, and carried to Rome by Scipio Nasica.

The stone considered as a symbol of Phœbus, and brought from Syria to Rome by Heliogabalus.

A stone preserved at Abydos, and another at Cassandria.

? Probably the symbol of Diana at Ephesus.

? Probably the black stone in the Caaba at Mecca, and another also preserved there.

(The stone preserved in the coronation-chair of the kings of England, and which was considered as something remarkable at a very remote period, is, according to late accounts communicated to me, not a meteoric production.)

B. *After the Christian Æra.*

A stone fell in the *Vocontorium agro*, perhaps in the first half or about the middle of the first century.

In the years 2, 106, 154, 310, and 333. Stones in China.

(The pretended fall of a stone at Constantinople in the year 416, mentioned by Sethus Calvisius, originated in a misunderstanding.)

452. Three very large stones in Thrace.

During the sixth century. Stones on Mount Lebanon, and near Emesa in Syria.

? 570 (or about that time). Stones near Beder in Arabia.

616. Stones in China.

? 618. A fiery stone at Constantinople.

? 839. Stones in Japan.

852, in July or August. A large stone in Tabaristan.

892 or 897 (or 908). At *Ahmed-Dad*, many stones.

951. A stone at Augsburg (not in Italy).

998. Two stones near Magdeburg.

Not long after 1009, a large mass of iron, according to the description similar to that of Pallas, at Dschordschan. (Subsequently the name of the place has been falsely read and written Cordova, and Lurgea, after a *Ro Torati* made of the sultan of Khorasan).

1021. Stones in Africa.

1057. A stone in Corea.

1112. Stones or perhaps iron, near Aquileja.

1135 or 1136. A stone near Oldisleben in Thuringia.

? 1138, the 8th of March. Probably stones near Mosul.

1164, during Whitsuntide. Iron in the district of Misnia.

(I pass over many accounts of that period, which are either fabulous, or relate perhaps to falls of hailstones).

1249, the 26th of July. Stones near Quedlinburg, &c.

? During the 13th century a stone is said to have fallen at Würzburg. (The specimen preserved there was nothing but an old battle-axe.)

Between 1251 and 1360, many stones fell near Welikoi-Usting in Russia.

1280.

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1280. Near Alexandria in Egypt, a stone or mass of iron.

1304, 1st of October. Near Friedland or Friedburg, many red-hot stones and masses of iron.

? 1328, 9th of January. Perhaps stones, in Mortahiah and Dakhaliah.

? 1339, 13th of July. Perhaps stones, in Silesia.

? 1368. Perhaps iron, in Oldenburg.

1379, 26th of May. Stones near Minden in Hanover.

1425. A meteoric stone in the island of Java.

? 1438. Near Roa in Spain, a great many very light stones.

1474. Near Viterbo, two large stones.—*Biblioteca Italiana*, tom. xix. p. 461, Sept. 1820.

? During the same century a stone seems to have fallen near Lucca, accompanied by a substance taken for coagulated blood.

1491, 22d of March. A stone near Rivolta de Bassi, not far from Crema.

* 1492, 7th November. The well-known fall of a large stone near Ensisheim.

1496, 26th or 28th of January. Stones between Cesena and Bertinoro, and in the vicinity of Forli.

? Perhaps during this century, or at the beginning of the following, a stone near Brussels.

(I forbear mentioning several accounts of that period in which the fall of hailstones seems to have been mistaken for that of meteorites.)

1511, 4th of September, or a few days after. A great fall of meteoric stones near Crema, not far from the river Adda. (Some authors, misunderstanding the words *prope Abduam*, have made Abdua of it.)

1516. In the province of Se-tschuan in China, six stones.

1520, in May. In Arragon, three stones.

? 1528, 29th of June. Large stones near Augsburg.

? 1540, 28th of April. A large stone and several smaller ones in Limousin.

Between 1540 and 1550. A large mass of iron in the forest near Neunhof, between Leipzig and Grimma. (Some authors have changed Neunhof into Neuholem.)

About the middle of the same century, iron in several parts of Piedmont.

1552, 19th May. A large fall of stones near Schleusingen, &c. (In several French and English periodicals Schleusingen has been confounded with Schleisheim near Munich.)

1559. Near Miskolz in Hungary, five large stones, or perhaps masses of iron.

1561, 17th May. Stones or masses of iron near Torgau and Eilenburg.

(There is an account of a fall of stones in 1564, the 1st of March, between Mecheln and Brussels, which seems to be fabulous.)

? 1572, 9th January. Perhaps a fall of stones near Thorn.

1580, 27th May. A large fall of stones near Nörten, not far from Göttingen.

1581, 26th July. A stone at Niederreissen near Buttelstädt in Thuringia.

1583, 9th January. A stone or mass of iron near Castrovillari in Abruzzo.

1583, 2d March. A stone in Piedmont.

1596, 1st March. Stones at Crevalcore in Ferrara.

Probably during the same century, a stone in the kingdom of Valencia in Spain.

1608, in the 2d half of August. In Styria, very large stones, together with a substance resembling blood.

1618. A metallic mass in Bohemia.

1621, 17th April. A mass of iron, near Lahore in India.

1622, 10th Jan. In Devonshire, a large stone.

1628, 9th April. In Berkshire, a stone.

1634, 27th October. In the county of Charollois, in the former duchy of Burgundy, a large fall of stones.

? 1635, 7th July. Perhaps a stone near Calce in the Vicentine.

1636, 6th March. A very large stone between Sagan and Dubrow, in Silesia.

1637 (not 1627), 29th November. A stone on Mount Vaisier in Provence.

1642, 4th August. A stone in Suffolk.

Between 1643 and 1644. Stones on-board a ship in the Indian Ocean.

1647, 18th Feb. A stone near Zwickau.

1647, in August. A fall of stones near Stolzenau in Westphalia.

? Between 1647 and 1654. A ball weighing eight pounds, and therefore probably a mass of iron, is said to have fallen on the deck of a vessel in the Indian Ocean, and to have killed two persons.

1650, 6th August. A stone at Dordrecht.

1654, 30th March. A large fall of stones on the island of Fuhnen.

About the middle of the same century, a large stone at Warsaw.

Likewise at Milan, a small stone which killed a Franciscan friar.

(An account of stones said to have fallen in 1667 at Shiraz seems to be fabulous).

1668 (not 1662, 1663, nor 1672), the 19th or 21st June. Very large stones in the Veronese.

1671, 27th February. Stones in the Ortenau in Suabia.

? 1673. Stones near Dietlingen in Baden. (Perhaps only the same event mistaken.)

1674, 6th October. Two large stones in the canton of Glarus.

? About 1675 or 1677. Near Copinsha, one of the Orkneys, a stone fell on a ship. (Perhaps a mistaken repetition of the former account.)

1677, 28th May. At Ermindorf near Grossenhain, stones differing from other meteoric stones, and which, according to their appearance, as well as to Balduin's analysis, contained copper, which some other circumstances render still more probable.

[The following instances are cited by Dr. Nœggerath in Schweigger's *Neues Journal,* Band. xiv. p. 357, from Beccher's *Laboratorium,* published in 1680: their dates are of course prior to that period.

Petermann Eterlein relates, in his Swiss Chronicle, that in a great storm a mass of iron fell from the heavens, together with a number of stones; and that the iron measured sixteen feet in length, fifteen in width, and two in thickness.

Paulus Merula says, in his *Cosmographia,* that six iron axes had fallen from heaven; upon which Beccher remarks that he does not believe them to have been really *axes,* but that they might have had the form of those weapons, as the stones which fall have, and whence they have received the name of *Donneräxte,* or *thunder-axes,* in the German language.—This relation seems doubtful, as the stone weapons of the aboriginal inhabitants of Europe have been called *thunder-bolts,* &c. in every language. Edit.]

(The account of the stones said to have fallen in 1686, the 18th of May, in London, near Gresham College, is to be erased from my work, page 239; since it appears from the work of Edward King, which I saw subsequently, at p. 20, that it was, like the event of 1791, nothing but hail. This instance, together with many others, proves how necessary it is not to trust to second-hand accounts, but always to refer to the first source.)

169[illegible], 13th January. Stones near Siena.

1698, 19th May. A large stone near Waltring, canton of Bern.

1706, 7th June. A large stone near Larissa in Thessaly.

1715, 11th April. Stones near Stargard in Pomerania. —Gilbert's Annals, vol. lxxi. (1822) p. 215.

1722, 5th June. Stones near the convent of Schefftlar in the district of Freissingen.

(The pretended fall of metal in 1731, near Lessay, was nothing but a misunderstanding of an electric phosphorescence of rain.)

1738, 18th October. A meteoric stone in the province of Avignon (badly described by a person ignorant of the subject).

1740, 25th October. Stones near Rasgrad on the Danube.

(The stone said to have fallen in Greenland, in the winter of 1740–41, was nothing but a piece of rock, which having detached itself from a hill, rolled down into the valley.)

1750, 11th October. Stones near Coutances, in the department de la Manche, or Normandy.

* 1751, 26th May. The well-known mass of iron near Hradschina in the province of Agram.

* 1753, 5th July. Stones near Tabor in Bohemia.

1753, in September. Two stones near Laponas in Bresse.

1755, in July. A stone near Terranova in Calabria.

1766, in the middle of July. A stone near Alboreto, not far from Modena.

? 1766, 15th August. Perhaps a stone near Novellara.

* 1768. A stone near Lucé, department de la Sarthe.

* 1768, 20th November. A stone near Maurkirchen in Bavaria.

1773, 19th September. A stone near Rodach in the duchy of Coburg.

1775 or 1776. Stones near Obruteza in Volhynia.

About 1776 or 1777, in January or February. Stones near Fabbriano.

1779. A fall of stones near Petriswood in Ireland, in the county of Westmeath.

1780, 11th April. Stones near Beeston in England.

1782. A large stone near Turin.

1785, 19th February. A fall of stones in the vicinity of Eichstädt.

* 1787, 1st October. Stones in the government of Charcow.

* 1790 (not 1789), 24th July. A very considerable fall of stones near Barbotan, Juliac, &c.

1791, 17th May. Stones near Castel-Berardenga in Tuscany.

(The account of a fall of stones on the 20th of October 1791, near Menabilly in Cornwall, mentioned in my work, page 261, must be expunged; since, according to the work of Ed. King, pp. 18 and 19, it was nothing but hail; as may also be seen by the drawing of some of the larger fragments.)

* 1794, 16th June. A well-known fall of many stones near Sienna.

1795, 13th April. Stones in Ceylon.

* 1795, 13th December. A stone near the Wold Cottage in Yorkshire.

1796, 4th January. A large stone near Belaja-Zerkwa in Southern Russia.

* 1798, 8th or 12th March. A stone near Sales, department of the Rhone.

1798, 13th December. Stones near Krakhut in the vicinity of Benares, in Bengal.

1801. On the Isle de Tonnelliers near the Mauritius.

1802, in the middle of September. In the Scotch Highlands†.

* 1803, 26th April. The well-known great fall of stones near L'Aigle, in the department de l'Orne or Normandy.

1803, 4th July. A fall of stones at East Norton in England, which did some damage.

1803, 8th October. Stones near Apt, in the department of Vaucluse.

* 1803, 13th December. Near Massing, district of Eggenfeld in Bavaria.

1804, 5th April. At High-Possil, near Glasgow, a stone.

1805, 25th March. Stones near Doroninsk in Siberia.

1805, in June. At Constantinople.

* 1806, 15th March. At Alais in the department du Gard, two stones differing from others by their friability, and also by containing 2·5 per cent of carbon, in addition to the usual constituents of meteoric stones.

1806, 17th May. A stone near Basingstoke in Hampshire.

* 1807, 13th March. A large stone near Timochin, in the government of Smolenskoi.

* 1807, 14th December. A fall of many stones near Weston in Connecticut.

* 1808, 19th April. Stones near Borgo San Donino, and in the duchy of Parma.

* 1808, 3rd September. Stones near Lissa in Bohemia.

? 1809, 17th June. Upon a ship, and in the sea, near the coast of North America.

1810, 30th January. Fall of stones in the county of Caswell in New Connecticut.

1810, about the middle of July. A stone near Shahabad in India. The meteor set five villages on fire, and injured several persons.

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† In a former catalogue of meteorites published in the Edin. Phil. Journ. vol. i. p. 230, we find the following note on this passage: "We have inserted this notice from Chladni, though we believe that no stones fell in Scotland at the time here mentioned."—Edit.

* 1810, in August. A stone in the county of Tipperary in Ireland.

* 1810, 23rd November. Three stones near Charsonville, near Orleans.

1811, between the 12th and 13th March. A stone in the government of Poltawa in Russia.

* 1811, 8th July. Some stones near Berlanguillas in Spain.

* 1812, 10th April. Stones near Toulouse.

* 1812, 15th April. A stone near Erxleben, between Magdeburg and Helmstadt.

* 1812, 5th August. A large stone near Chantonay, department de la Vendée, which differs from others in having no crust, and in a few other particulars.

1813, 13th March. Meteoric stones near Cutro in Calabria, attended with a remarkable fall of red dust in several parts of Italy.

? 1815, in the summer. Stones are said to have fallen near Malpas in Cheshire.

* 1813, 10th September. Stones in the county of Limerick in Ireland.

1814, 3rd February. In the district of Bachmut in Russia, government of Ekaterinoslaw.

1814, about the middle of March (or 1813, 13th December). Stones near Sawotaipola or Sawitaipal in Finland.—Vide my work, and Schweigger's *Neues Journ.* Band i. p. 160.

* 1814, 5th September. Many stones near Agen, department du Lot et Garonne.

1814, 5th November. Stones in the Doab in the East Indies.

1815, 18th February. A stone near Duralla in India.—Phil. Mag., August 1820, p. 156. Gilbert's Ann., vol. lxviii. p. 333.

* 1815, 3rd October (not the 30th). A fall of stones near Chassigny, not far from Langres in Champaigne, or department de la Haute Marne. They belong to that class of meteorites which contain no nickel, and are further distinguished by their greater friability, greenish-yellow colour, glimmering appearance, and a crust as if varnished.

A stone is said to have fallen a few years ago, in the Isle of Man, very light and of a scoriaceous texture.—Phil. Mag. July 1819, p. 39.

1816. A stone near Glastonbury in Somersetshire.

(I pass over several other accounts of pretended falls of stones, as being unfounded.)

1818. 10th August. A stone near Slobotka, government of Smolensko in Russia.

? 1819. Towards the end of April a meteoric fall seems to have taken place near Massa Lubrense, in the Neapolitan duchy of Salerno, which appears not to have been sufficiently attended to.

1819, 13th June. Stones near Jonzac, department de la Charente inferieure.—*Journ. de Phys.* Fev. 1821, p. 136. *Mém. du Museum d'Hist. Nat.* tom. vi. p. 233. Thomson's Ann. of Phil. Sept. 1820, p. 234. *Neues Journ. f. Chem. u. Phys.* vol. xxix. No. 4, p. 508.

* 1819, 13th October. A stone near Politz, not far from Gera or Köstritz, in the principality of Reuss in Germany (not in Russia, as was stated in Thomson's Annals, and repeated in several French publications).—*Neues Journ. fur Chem. u. Phys.* vol. xxvi. No. 3, p. 243. Gilbert's Ann. vol. lxiii. pp. 217 and 451.

? 1820. In the night between the 21st and 22d of May, a small stone is said to have fallen at Oedenburg in Hungary. *Hesperus*, vol. xxvii. No. 3, p. 94.

* 1820, 12th (not 19th) July. A fall of stones in the circle of Dunaburg in Courland, of which an analytic account and a drawing has been given in Gilbert's Ann. vol. lxvii. No. 4, p. 337, by Baron Th. von Grotthuss; and I am indebted to the kindness of that gentleman for a fragment of this stone, which differs from others, in its possessing a larger proportion of iron.

1821, 15th June. Fall of one large and several small stones near Juvenas, in the department de l'Ardèche, of which an account made up from those that had been given in the *Ann. de Chim.*, together with Vauquelin's and Laugier's analyses, appeared in Gilbert's Annals, vol. lxix. p. 407, &c., and vol. lxxi. pp. 201 and 203.

1822, 4th June. A fall of stones near Angers.

[1822, 13th September. A stone fell in the vicinity of Epinal in the department of the Vosges, in France.—*Ann. de Chim. et de Phys.* tom. xxi. p. 324.

1823, 7th August. Stones fell at Nobleborough in the state of Maine, U. S.—Phil. Mag. vol. lxiii. p. 16.

1824, 15th January. Stones fell in the commune of Renalzo, province of Ferrara, in Italy.—Ferrussac's *Bulletin*, sect. i. Sept. 1825, p. 183.

1824. Early in March, stones are said to have fallen near the village of Arenazo, in the legation of Bologna. Phil. Mag. vol. lxiii. p. 233.—Is this a mistaken notice of the preceding?

1825, 10th February. A stone weighing sixteen pounds seven ounces fell at Nanjemoy in Maryland, U. S.—Annals of Philosophy, N.S. vol. x. p. 186.]

§ III. *Masses of Native Iron containing Nickel, which are to be considered as meteoric.*

A. *Spongy or cellular, the interstices being filled with a Substance resembling Olivine.*

* The large mass found in Siberia, and made known by Pallas, whose meteoric origin was known to the natives, and in which the iron and olivine have the same constituents as are found in meteoric stones†.

? A fragment found between Eibenstock and Johann Georgenstadt.

One in the imperial cabinet of natural history at Vienna, said to have been brought from Norway.

* A mass weighing several pounds, found in a field, probably at Grimma in Saxony, in the ducal cabinet of natural history at Gotha ‡.

(The mass which fell in Dschordschan soon after the year 1009, according to the description must have been of this kind.)

B. *Solid Masses of Iron containing Nickel, and crystallized in Octahedrons.*

(The only mass yet in existence, whose fall may be considered as being historically proved, is that which fell in the province of Agram in 1751, as mentioned above. The following, however, we conclude to be such, from their conformity with this and other circumstances.)

* The mass preserved in Bohemia, from time immemorial, under the name of the *Enchanted Burggraf*, the greater part of which is now in the cabinet at Vienna. The name, as well as the remains of a tradition, in which a tyrannical nobleman is said to have been killed by it, in the suburbs of Hrabicz, lead us to suppose that its fall had actually been noticed.

* The mass found near Lenarto in Hungary, on the boundary of Gallicia, in which on the surface, treated with acids, as well as in the fracture, the crystalline texture very distinctly appears.

* One or several masses found at the Cape of Good Hope.

Many masses, and among them several large ones, on the right bank of the Senegal.

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† Being unacquainted with any account of the crystallization of the olivine or peridot in this mass, it may not be improper to remark that I have one piece, of the size of a pea, beautifully crystallized in the form of a pentagonal dodecahedron, besides several other pentagonal crystallized surfaces being observable in it.—[See Phil. Mag. vol. lxvi. p. 356.—EDIT.]

‡ Ibid. p. 367.

* Several large and small masses in Mexico and in the Bay of Honduras.

* A very large mass near Otumpa in the district of Santiago del Estero, in South America†. Another, on the left bank of the Rio de la Plata, is said to be still larger.

* A very large mass, about fifty Portuguese miles from Bahia in Brazil; respecting which may be seen, besides the authorities mentioned in my own work, the account of the Bavarian naturalists Martius and Spix.

A mass found near the Red River in America, and brought to New York.

Two masses on the northern coast of Baffin's Bay.

A mass found near Bitburg, to the north of Treves, which has been probably smelted. (I have mentioned it in my book, p. 353, as doubtful, not knowing then, as I have since learned from the American Mineral ogical Journal, vol. i. p. 218, that after an analysis by Colonel Gibbs, it was found to contain nickel, and to be in every respect similar to the mass at New York.)‡

A mass discovered by Professor Horodecki of Wilna, near Rockicky, district of Mozyrz, in the government of Minsk, in which Laugier found nickel and a little cobalt.—Gilbert's Annals, vol. lxiii. p. 32.§

[Many masses of different sizes, discovered about the year 1810, in the vicinity of Santa Rosa, in the eastern Cordillera of the Andes; and which probably belong to this division.—Edin. Phil. Journ. vol. xi. p. 120.

Two masses discovered at Zipaquirá, in the same Cordillera. Ibid. p. 122.]

? It is possible that the isolated rock of forty feet high, near the source of the Yellow River, in Eastern Asia (according to Abel-Remusat's account in the *Journ. de Phys.* May 1819) is of this description. The Moguls say that it fell down from heaven; and they call it *Khadasoutsilao*, i. e. rock of the pole.

* The oldest fragment of meteoric iron, the antiquity of which can be historically proved, is probably the antique mentioned in my work, p. 390, for which I am indebted to Professor Rösel of the Academy of the Fine Arts at Berlin, in whose presence it was dug up at Pompeii, near the temple of Jupiter, and the Goldsmith's-street, in 1817. Its external texture even shows it to be meteoric; and being protoxidated from its having lain so long in the damp volcanic sand, it is no longer attracted by the magnet, but still acts on the

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† See Phil. Mag. vol. lxvi. p. 367.—Edit.

‡ Ibid. vol. lxv. p. 401.

§ Ibid. p. 411.

magnetic needle. It is a rounded oval about a quarter of an inch long, and a little less in breadth, and seems intended to have been set in a ring. One end is a little broken off. One side is a little more convex than the other, on which a small elliptic slab of jasper of a reddish brown is let in; and on this a star and a moon by the side of it are engraved. As the ancients considered substances fallen from heaven (*Bætylia*) as something sacred (upon which subject see the works of Münter and Fred. von Dalberg), and as on several coins, &c. the meteoric origin has been indicated by a star†, it probably indicates that this iron fell down with a fiery meteor of the apparent size of the moon. Now it seems more probable that it is a part of the iron which fell in Lucania, about fifty-six or fifty-two years before Christ, as mentioned by Pliny, *Hist. Nat.* ii. 57, than of any other: 1st, because it was close to Pompeii; 2dly, because no other fall of iron is mentioned by any more ancient author; and 3rdly, because the destruction of Pompeii occurred only about 135 years after that fall, which would therefore be still in the recollection of the people.

C. *Masses of Native Iron, the Origin of which is uncertain, being different in Texture from the former, and containing no Nickel.*

* The large mass at Aix-la-Chapelle, containing a little arsenic, silicium, carbon, and sulphur. It may possibly be the produce of the furnace; against which hypothesis, however, many objections might be made.

* A mass found in the Milanese, on the Collina di Brianza, near Villa, weighing between 200 and 300 pounds, of very pure iron, with a small trace of manganese and sulphur. The texture is spongy, and the iron whiter than usual, and exceedingly malleable; on which account it cannot be considered as a product of the furnace.

A mass found near Gross-Kamsdorf, in 100 parts of which Klaproth found 6 of lead and 1·50 of copper. The fragment possessed by him (a part of which is now in the cabinet of natural history at Vienna), as well as the specimen in the museum at Paris, may be considered genuine; but the fragments shown at Freiburg and Dresden are unquestionably spurious.

Some other masses (for instance, that found near Florac) must be considered as products of artificial fusion.

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† To this method of indicating the fall of a fiery meteor, the Chinese expression, "A star fell to the earth, and turned into a stone," bears a close analogy.

IV. *Fallen Substances, not being Meteoric Stones or Native Iron, but which in every appearance and in the most essential points agree with Meteoric Stones.*

(Livy iii. 10, mentions that about 459 years before our æra, flesh fell from the sky, which was partly caught up by birds in the air, and when on the ground, lay for many days without putrifying. If this story be not altogether an invention, it is difficult to guess what could have given rise to it.)

About the year 472 of our æra, on the 6th of November, or as some say, the 5th or 11th, there was, probably in the vicinity of Constantinople, a fall of a great quantity of a mephitic black dust, accompanied by fiery meteors, which led people to apprehend the destruction of the world.

652. Also a fall of dust near Constantinople, which excited terror.

743. A fall of dust in several places, accompanied by a meteor.

During the middle of the ninth century, blood-coloured dust, in several places.

929. At Bagdad, a reddish sand, after a red appearance in the sky.

1056. In Armenia, red snow.

1110. In Armenia, the fall of a fiery meteor into the lake Van, with much noise, and by which the water turned to a blood-colour; and deep rents were found in the earth.

1222. Red rain near Viterbo.—*Biblioteca Italiana*, tom. xix. p. 461.

1416. Red rain in Bohemia.

? Probably during the fifteenth century, at Lucerne, a liquid like coagulated blood, and a stone with a fiery meteor.

1501. Red rain in several places.

1543. Red rain [illegible] Westphalia.

1548, 6th November. Probably in the district of Mansfeld, the fall of a substance, like congealed blood, attended by a fireball and great noise.

1557. Friday after Sexagesima, at Schlage in Pomerania, large pieces of a substance resembling congealed blood.

1560, or 1568, or 1571, at Whitsuntide. Red rain in the vicinities of Löwen and Emden.

1560, 24th December. At Lttebonne, department de la Seine Inferieure, red rain with a fiery meteor.

? 1562, 5th July. At Stockhausen, a German mile from Erfurth, a fall of a substance resembling hair, attended by a commotion and extraordinary noise.

1586,

1586, 3rd December. At Verden in Hanover, and other parts, a great quantity of a blood-red and blackish substance, by which a plank was burnt, attended by a thunder-storm: (meteors and reports).

1618, in the second half of August. A fall of large stones attended by a fiery meteor, and what is called a rain of blood, in Styria.

1623, 12th August. Rain of a blood-colour at Strasburg, subsequent to the appearance of a thick red-smoke-coloured cloud.

1637, 6th December. From seven o'clock in the evening till two on the following day, a great fall of black dust in the Gulf of Volo, in the Archipelago, and near Acra in Syria.

1638. Red rain near Tournay.

? 1642, in June. At Magdeburg, Lohburg, &c., large lumps of sulphur.

1643, in January. Rain called a rain of blood, at Vaihingen and Weinsberg.

1645, between the 23d and 24th January. Red rain near Herzogenbusch.

1646, 6th October. At Brussels.

1652, in May. Between Siena and Rome, a transparent, slimy, and adhesive substance, in the place where a very bright meteor had been seen to fall.

? 1665, 23rd March. Near Laucha, not far from Naumburg, a substance like dark blue silk threads, in great quantity.

? 1665, 19th May. In Norway, with an uncommon thunder-storm (or a meteor mistaken for such), sulphureous dust.

1678, 19th March. Red snow near Genoa.

* 1686, 31st January. Near Rauden in Courland, a black substance like paper, in great quantity: a similar substance is said to have fallen at the same time in Norway and Pomerania. Baron Th. von Grotthuss found a portion of it in an old cabinet of natural curiosities, and has published his analysis of and interesting observations on it, in Schweigger's *Journal*, Band xxvi. p. 332, &c. He has been kind enough to present me with a fragment of it.

1689. At Venice, and in the vicinity, red dust.

1691. Red rain at Orleans, à la Madelaine, according to Lemaire.

1711, 5th and 6th May. Red rain near Orsio in Schonen.

1781, 24th March. On the Island of Lethy, a heap of a jelly-like substance on the spot where a fiery meteor had fallen with a report.

1719. A rain of dust with a radiant appearance, on the

Atlantic Ocean, under 45° N. latitude, and 322° 45′ longitude.

1721, in the middle of March. What was called rain of blood, at Stuttgard, with a meteor.

1737, 21st May. Fall of earth, which was entirely attracted by the magnet, on the Adriatic sea, between Monopoli and Lissa.—*Giov. Jac. Zanichelli,* in the sixteenth volume of the *Opuscoli di Calogera.*

1742. Red rain at San Pies d'Arena, near Genoa.

1755, in October and November. In a great many places at a great distance from one another, a fall of red and black dust, with or without rain.

1762, in October. At Detroit in North America, an extraordinary darkness from before daybreak till four o'clock in the afternoon, with rain containing sulphur and a black substance.—Phil. Trans. vol. liii. p. 519.

1763, 9th October. Red rain in the duchy of Cleves, and near Utrecht.

1763, and likewise 1765, 14th January. Red rain in Picardy.

1781, 24th April. In the Campagna di Noto, in Sicily, a whitish dust, which was not volcanic.

* 1796, 8th March. With an exploding fire-ball seen in a great part of North Germany, an adhesive gummy mass, in Upper Lusatia, not far from Bauzen.

Without being able to fix the time. Near Crefeld, a jelly-like substance, after the fall of a mass of fire.

1803, from the 5th to the 6th of March. In Italy, red dust that was not volcanic, partly with rain or snow, and partly without, coming from the south-east, and exciting great terror.

1809. Red rain in the Venetian territory.

1810, 17th January. Near Piacenza, red snow, with lightning and thunder-claps (probably a fiery meteor exploding).

1811, in July. Near Heidelberg, fall of a slimy substance with an exploding fire-ball.—Gilbert's Annals, vol. lxvi. p. 329.

1813, 13th and 14th March. In Calabria, Tuscany, and Friuli, a great fall of red dust and red snow, with much noise, attended by fiery meteors and the fall of stones, near Cutro in Calabria. The component parts of this dust were nearly the same as in the meteoric stones that do not contain nickel.

1814, 3rd and 4th July. A great fall of black dust with appearances of fire, in Canada, near the mouth of the river St. Laurence. The event is very similar to that of the year 472.

1814, in the night between the 27th and the 28th October. In the valley of Oneglia in the Genoese territory, a rain of red earth.

1814, 5th November. Every meteoric stone that fell near the Doab in India was surrounded by a small heap of dust.

? 1815, towards the end of September. A probable fall of dust in the Southern Indian Ocean, an extent of more than 50 miles in diameter having been found covered with it.

1816, 15th April. Tile-red snow from red clouds, in some parts of Northern Italy.

1818. Captain Ross found red snow on the north coast of Baffin's Bay. Notwithstanding the very defective analysis (in which it was supposed, from ignorance of the analyses previously made of red meteoric dust, that the colouring matter must be the excrement of certain birds), they found, besides other substances, oxide of iron and silica, but which, owing to the false preconception, they considered as something adventitious. The oxide of iron seems to be the principal colouring substance; and the kind of mould called *uredo nivalis*, which was found by the microscope in the long-preserved snow-water, was probably of an infusorial nature, and produced in it at a subsequent period.

* Red snow was also found in 1817, on Mount Anceindaz in the south-east of Switzerland, by M. de Charpentier, director of the salt-manufactory of Bex, who was so kind as to give me the residue he collected from a flat rock; which, however, seems to have been mixed with some fragments of lichen. Professor Steinmann in Prague, and Professor Ficinus in Dresden found in it (as had been found in other meteoric dust), besides a volatile substance which leads us to infer the presence of some organic matter, oxide of iron, manganese, silica, alumina, lime, and a little sulphur. Prof. F. discovered also a trace of lime, but no traces of nickel, chrome, or cobalt. I have given some account of this in Gilbert's Annals, vol. lxviii. p. 356; also in my own work.

Accounts and an analysis of red snow found on mount St. Bernard (the colouring of which might possibly have been effected by lichen or dust containing iron being carried there by the wind) may be found in Gilbert's Annals, vol. lxiv. p. 319, as extracted from the *Bibliothèque Universelle*, besides some other notices on red dust. (It is very desirable that *black meteoric dust* should be accurately analysed.)

1819, 13th August. At Amherst in Massachusetts, the fall of a mephitic slimy substance attended by a fiery meteor. Silliman's Journal of Science, vol. ii. p. 335. (A more exact analysis of this substance would, however, be very desirable).

1819, 5th September. At Studein in the lordship of Keltsch, in Moravia, a fall of dry earth from a bright cloud in a clear sky.—*Hesperus*, 1819. *Nov. Beilage*. No. 42.

1819, 5th November. Red rain in Holland and Flanders, according to the *Ann. Génér. des Sc. Phys.* It is not surprising that cobalt and muriatic acid were found in it by analysis, since both these substances have been found in meteoric stones.

1819, in November. Near Montreal and in Maine, during an unusual darkness, black dust with an appearance of fire, and noise; whence it may be seen that it was not, as some pretend, the result of the burning of a forest, but of a meteoric nature. Accounts of it are given in the American and English Journals, and repeated in Gilbert's Annals, vol. lxvii. pp. 187 and 218, and vol. lxviii. p. 354.

? 1820, in the beginning of October. Near Pernambuco in Brasil, and on the sea, a substance like silk, in great quantities. —Vide *Annales de Chim.* tom. xv. p. 427; where a chemical analysis is promised.

1821, 3rd May. Red rain at and near Giessen, during a calm, from a moderate-sized *stratus*, as detailed in the newspapers. Professor Zimmermann of that town found it to contain, upon a hasty analysis, chromic acid, oxide of iron, silica, lime, a trace of magnesia, carbon, and several volatile substances, but no nickel.

This gentleman, according to newspaper accounts, has found in the common rain which has fallen for some time past several substances which are found in meteoric stones; even iron containing nickel. However interesting these investigations may be, they furnish nothing decisive towards the hypothesis of fire-balls and other masses which have fallen on our earth being the produce of this planet, since it is very possible that the bodies contained in the rain were brought into the atmosphere by the uncommonly great number of fiery meteors that have lately appeared *. Even if the greater part of our atmosphere consisted of such substances, or could be transformed into such by some *Deus ex machinâ*, such meteors, as well as shooting stars, cannot be atmospherical; since their *course and velocity*, which have been so frequently determined by observations from different stations, and calculations of their parallax, are sufficient to evince their cosmical origin as *mathematically proved*. If therefore any one can yet doubt, it is like persons perfectly ignorant of the subject affecting to doubt the correctness of our astronomical and cosmological knowledge. It is however easier to form a partial opinion of things, than to take proper notice of what has been done by others. I have assembled the results of all existing observations on the height, velocity, and movement of fire-balls in the 2d, 3rd, and 4th division of my work, which ought to be known previously to a person's forming an opinion on the origin of meteors. Besides, having mentioned with every phænomenon the sources whence I took my account, the further details may be easily found by referring to them; and finally, I have in the last division of the work drawn together the results of them, by which the proofs of their cosmical origin, and of the impossibility of their being the produce of our earth or our atmosphere, are elucidated in the simplest and most natural manner.

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* I have given all the observations I could obtain of the meteors which have lately appeared, especially those of last winter, in Gilbert's Annals, vol. lxxi. No. 4 (1822, No. 8). I regret that from many parts of the world similar accounts are withheld.

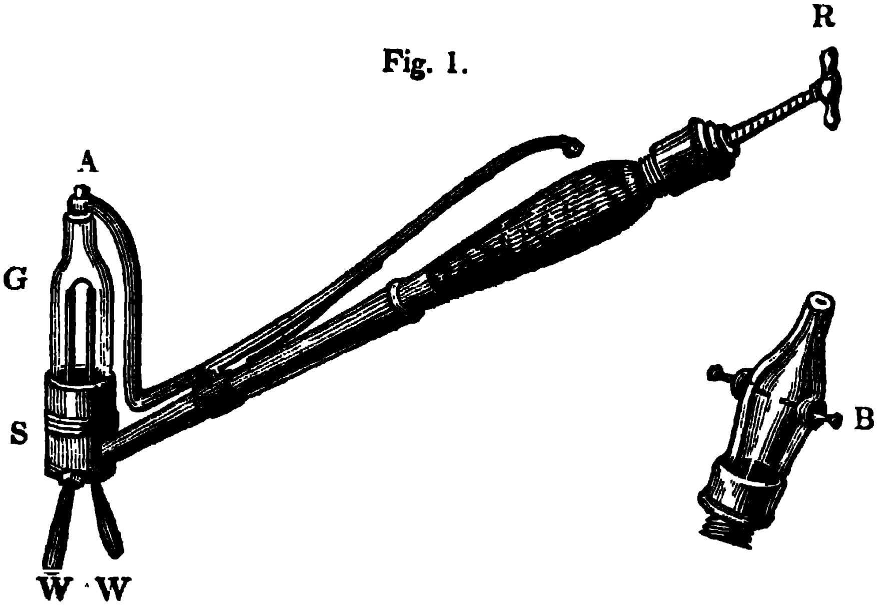
II. *An Account of some Eudiometers of an improved Construction. By* Robert Hare, *M.D. Professor of Chemistry in the University of Pennsylvania.*

IN the second volume of the American Journal of Science I published an account of some eudiometers, operating by a mechanism which, previously, had not been employed in eudiometry. A graduated rod, sliding into a tube through a collar of leathers soaked in lard, and compressed by a screw so as to be perfectly air-tight, was employed to vary the capacity of the tube, and at the same time to be a measure of the quantity of air, or of any other gas, consequently drawn in or expelled. About one-third of the tube was occupied by the sliding rod. The remainder, being recurved and converging to a perforated apex, was of a form convenient for withdrawing measured portions of gas from vessels inverted over water or mercury.

There were two forms of the sliding rod eudiometer: one designed to be used with nitric oxide, or with liquids absorbing oxygen: the other, with explosive mixtures. The latter differed from the eudiometers for explosive mixtures previously invented, in the contrivance for exploding the gases, as well as in the mode for measuring them; a wire ignited by galvanism being substituted for the electric spark, as the means of inflammation.

I shall proceed to describe several eudiometers, operating upon the principle of those above alluded to, with some modifications suggested by experience. Fig. 1 represents a hydro-oxygen eudiometer, in which the measurements are made by a sliding rod, and the explosions are effected by the galvanic ignition of a platina wire, as in an instrument formerly described, excepting that the method then employed of cementing the platina wire, in holes made through the glass, having proved insecure, a new and unobjectionable method has been adopted.

Fig. 1.



In the instrument represented by the preceding cut, the igniting wire is soldered into the summits of the two brass wires (WW), which pass through the bottom of the socket (S), parallel to the axis of the glass recipient (G), within which they are seen. One of the wires is soldered to the socket; the other is fastened by means of a collar of leathers packed by a screw, so that it has no metallic communication with the other wire, unless through the filament of platina, by which they are visibly connected above, and which I have already called the igniting wire. The glass has a capillary orifice at the apex (A), which by means of a lever and spring (apparent in the drawing) is closed, unless when the pressure of the spring is counteracted by one of the fingers of the operator. The sliding rod (seen at R) is accurately graduated to about 520 degrees.

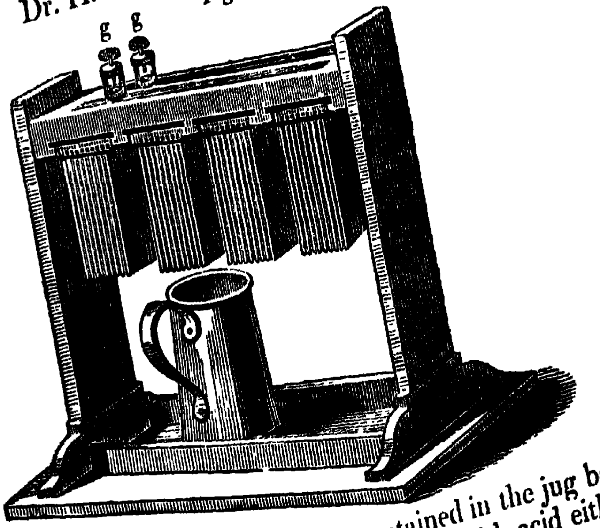
So easy is it to manipulate with this instrument, that any number of experiments may be performed in as many minutes. The ignition of the platina wire is caused by either of four calorimotors, each consisting of four plates of zinc, and five of copper. They are all suspended to one beam, as may be seen in fig. 2 following.

Two furrows are made in the wood of the beam, one on each side. These are filled by pouring into them melted solder, after having caused a metallic communication between one furrow and all the copper surfaces of all the four calorimotors; also between all their zinc surfaces and the other furrow.

The

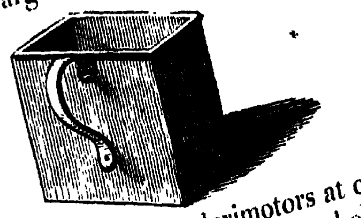
Dr. Hare's *improved Eudiometers.*

Fig. 2.



The acid for exciting the plates is contained in the jug below, which may be so uplifted as to surround with acid either of the calorimotors. Hence while one is in operation, the others are, by repose, recovering their igniting power. Or by using a vessel (fig. 3) large enough to receive, and containing acid

Fig. 3.



enough to excite two of the calorimotors at once, the igniting power may be doubled. The vessels for holding the acid are made of copper, covered with a cement of resin rendered tough by an adequate admixture of mutton suet.

In order to use the eudiometer, it must be full of water, and free from air-bubbles, and previously proved air-tight*, the rod being introduced to its hilt, and the capillary orifice open, in consequence of the pressure of the finger on the lever by which it is usually closed. Being thus prepared, let us suppose that it were desirable to analyse the atmosphere. Draw out the rod 200 measures; a bulk of air, equivalent to the portion of the rod thus withdrawn, will of course enter at the capillary opening; after which the lever must be allowed to close it. Introduce the recipient into a bell-glass of hydrogen, and opening the orifice draw out the rod about 100 degrees; close the orifice, and withdraw the instrument from the water. Apply the projecting wires (WW) severally to the solder in the two furrows in the beam (fig. 2) communicating with the poles of the four calorimotors; then raise the jug so as that it may receive one of them, and subject it to the acid. By the consequent ignition of the wire, the gas will explode. The instrument being plunged again into the water of the pneumatic cistern, so that the capillary orifice, duly opened, may be just below the surface; the water will enter and fill up the vacuity caused by the condensation of the gases. The residual air being excluded by the rod, the deficit will be equivalent in bulk to the portion of the rod remaining without; and its ratio to the air subjected to analysis may be known by inspecting the graduation.

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* To prepare the instrument and prove it to be in order, depress the glass receiver below the surface of the water in the pneumatic cistern, the capillary orifice being uppermost and open; draw the rod out of its tube, and return it alternately, so that at each stroke a portion of water may pass in, and a portion of air may pass out. During this operation, the instrument should be occasionally held in such a posture as that all the air may rise into the glass recipient, without which its expulsion by the action of the rod is impracticable. Now close the orifice (at the apex A) and draw out a few inches of the rod, in order to see whether any air can enter at the junctures, or pass between the collar of leathers and the sliding rod. If the instrument be quite air-tight, the bubbles extricated in consequence of the vacuum produced by withdrawing the rod will disappear when it is restored to its place. This degree of tightness is easily sustained in a well-made instrument.

In the case of the gaseous mixtures above described, the deficit has, in my experiments, been 126 measures. Whereas, according to the theory of volumes, it ought to be only 120. But I have not as yet operated with hydrogen purer than it may be obtained from the zinc of commerce; and some allowance must be made for the carbonic acid of the air, which may be condensed with the aqueous vapour produced by the oxygen and hydrogen.

In the invaluable work on the Principles of Chemistry, lately published by Dr. Thomson, it is suggested, that in order to obtain correct results in analysing the air with the hydro-oxygen eudiometer, more than 42 per cent of hydrogen should not enter into the mixture. I am not as well satisfied of the correctness of this impression, as I am generally with the results of the wonderful industry and ingenuity displayed in the work above mentioned.

If oxygen is to be examined by hydrogen, or hydrogen by oxygen, we must of course have a portion of each in vessels over the pneumatic cistern, and successively take the requisite portions of them, and proceed as in the case of atmospheric air.

B (fig. 1) represents a glass with wires inserted through small tubulures, in the usual mode for passing the electric spark, should this method of producing ignition be deemed desirable for the sake of varying the experiments, or for the purpose of illustration. This glass screws on to the socket (S), the other being removed. The wires (WW) remain, but should be of such a height as not to interfere with the passage of the electric spark. The instrument is operated with as usual, excepting the employment of an electrical machine, or electrophorus, to ignite the gaseous mixture, in lieu of a calorimotor. For the travelling chemist the last-mentioned mode of ignition may be preferable, because an electrophorus is more portable than a galvanic apparatus.

In damp weather, or in a laboratory where there is a pneumatic cistern, or amid the moisture arising from the respiration of a large class, it is often impossible to accomplish explosions by electricity.

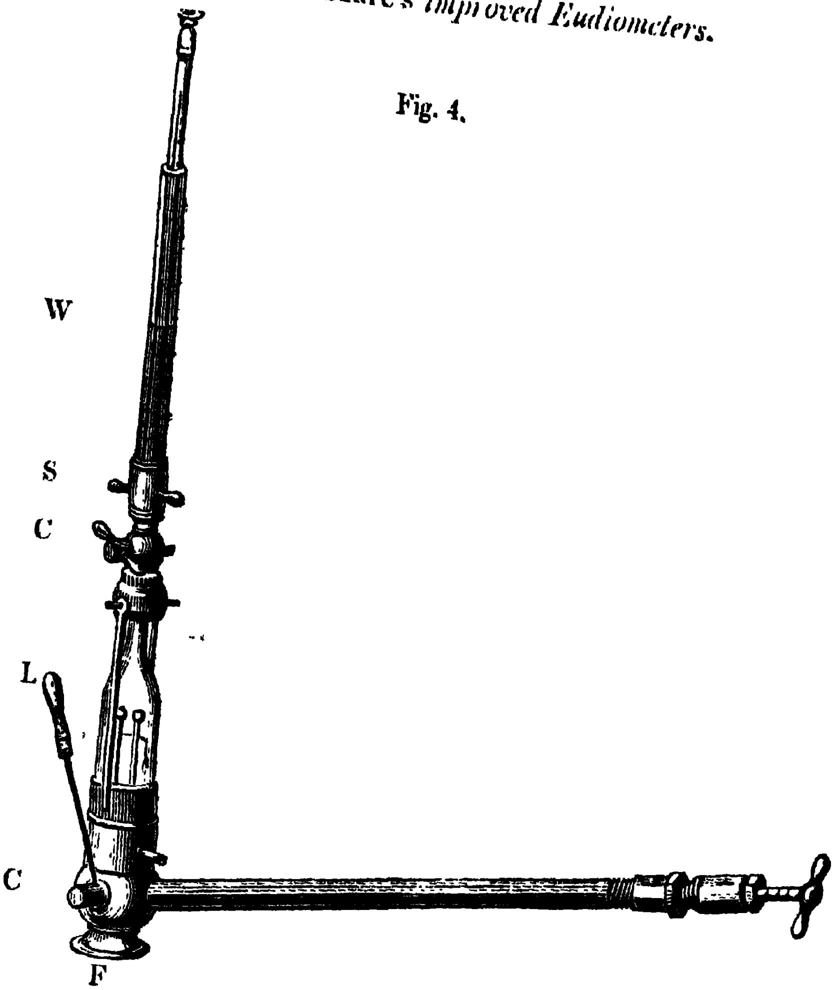
Of the Mercurial Sliding-Rod Eudiometer with a Water Gauge.

The eudiometer which I have described, though satisfactory in its results, and in its conveniency, when used with water, has not been found so when used over mercury. The great weight of this fluid caused the indications to vary in consequence of variations of position, during manipulation, too slight to be avoided. The instrument represented in the following cut (fig. 4) is furnished with a water gauge, which being appealed to, enables us to render the density of the gases within *in equilibrio* with the air without. Hence we can effect their measurement with great accuracy.

Let us suppose that this eudiometer has been thoroughly filled with mercury, the sliding-rod being drawn out to its greatest extent, and that it is firmly fixed over a mercurial cistern in the position in which it is represented in the drawing, the little funnel-shaped part at the bottom descending into the fluid to the depth of half an inch. Above this part is seen a cock (C), the key of which, in addition to the perforation usual in cocks, has another, at right angles to, and terminating in, the ordinary perforation. When the lever (L) attached to the key of this cock is situated as it is seen in the drawing, the tube containing the sliding rod communicates with the recipient, but not with the mercury of the reservoir. Supposing the lever moved through a quarter of a circle, to the other side of the glass, the tube in which the rod slides will communicate at the same time with the recipient and the reservoir. By means of the gauge-cock (C) the passage between the gauge and the recipient is opened or shut at pleasure.

As subsidiary to this eudiometer, another is provided with

Fig. 4.



a rod and graduation exactly similar*, excepting its being shorter. (See fig. 5.)

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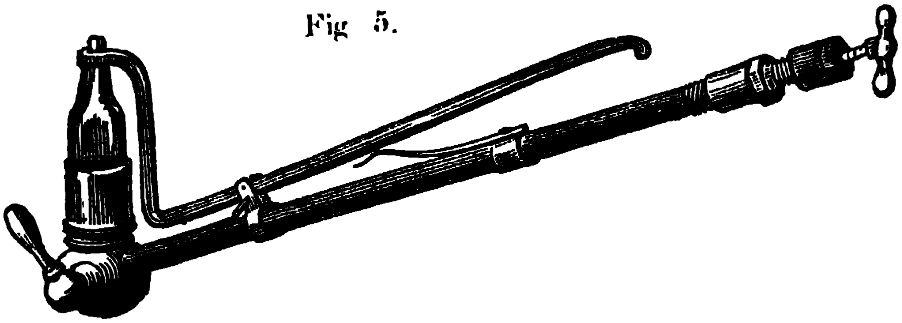
* In order to ensure accuracy in the measures of gas, made by the subsidiary eudiometer, it is necessary to attend to the following precautions. In the first place, the instrument must be proved air-tight, and free of air-bubbles, by the means prescribed already in the case of the eudiometer for water. (See note, page 23.) The presence of air-bubbles is always indicated by the extent of the vacuity which appears when the glass recipient is held uppermost, and which disappears when it is held lowermost: the weight of the mercury acting upon the elasticity of the tubes always causes a minute change; but by the smallest bubbles of air the effect is very much augmented. The eudiometer should be introduced into the vessel whence the gas is to be taken, and about ten per cent more than is necessary drawn in by opening the orifice and duly drawing out the rod. The eudiometer being lifted from the mercury with as little change of position as possible, the rod may be adjusted accurately to the point desired. A momentary opening of the orifice causes the excess to escape. The gas thus measured and included is then easily transferred to the principal eudiometer, by introducing the apex of the subsidiary instrument under the funnel (see F, fig. 4), opening the orifice, and forcing the sliding rod home.

Fig 5.



The method of analysing atmospheric air by means of these instruments is as follows. Supply the subsidiary eudiometer with its complement of hydrogen gas, by introducing the apex of the glass recipient into a bell-glass containing, over mercury, the gas in question, and drawing out the sliding-rod, the orifice being kept open only while above the surface of the mercury and inside of the bell.

The gauge-cock (C, fig. 4) of the principal eudiometer being closed, and that which opens a communication between the recipient and the funnel (F) open, and the instrument having been previously thoroughly filled with mercury, and placed over the mercurial cistern, as already mentioned, introduce into it, through the funnel, the gas which had been included in the subsidiary instrument (fig. 5); next shut off the communication with the mercurial cistern, re-establish those between the recipient and the rod and gauge, and push the rod into its tube up to the hilt. The re-entrance of the rod, by raising the mercury into the recipient, forces the hydrogen in bubbles through the water of the gauge, and displaces all the atmospheric air which it previously contained. Now shut the passage to the gauge, open that which communicates through the funnel with the mercurial cistern, and draw out the rod to its utmost extent. Into the eudiometer thus situated and prepared, introduce successively 100 measures of hydrogen and 200 measures of atmospheric air, by means of the subsidiary eudiometer: then closing the passage to the mercurial cistern, and opening the passage to the gauge, push in the rod until the water in the gauge indicates that the pressure on the gases included is equivalent to that of the external air. The gauge-cock being closed, the gases are ready to be exploded. The explosion is produced by galvanic ignition, as in the case of the eudiometer for water (fig. 1), excepting that instead of carrying the eudiometer to the calorimotor, the circuit is established by lead rods severally attached to the galvanic poles by gallows and screws. (See g g, fig. 2.) One of the lead rods terminates in a piece of iron immersed in the mercury, the other is fastened to the insulated wire of the eudiometer. Under

der these circumstances, one of the calorimotors is surrounded with the acid contained in the jug, and an explosion almost invariably succeeds. Before effecting the explosion, the number of the degrees of the sliding-rod which are out of its tube should be noted; and it must afterwards be forced into the tube, in order to compensate the consequent condensation of the gases as nearly as it can be anticipated. A communication with the gauge must then be opened gradually. If the water is disturbed from its level, the equilibrium must be restored by duly moving the rod. Then deducting the degrees of the sliding-rod remaining out of the tube from those which it indicated before the explosion, the remainder is the deficit caused by it; one-third of which is the quantity of oxygen gas in the included air. Or, the residual air being expelled by the rod, and the quantity thus ascertained deducted from the amount included before the explosion, the difference will be the quantity condensed.

It may be proper to mention, that as other metals are almost universally acted upon by mercury, the cocks, sockets, screws, and sliding-rods of the mercurial eudiometers are made of cast steel. The tubes containing the rods are of iron.

Since the drawings (figs. 1 and 4) were made, verniers have been attached to the screws through which the sliding-rods pass; so that the measurements are made to one-tenth of a degree.

I have alluded to the water-gauge without explaining its construction. It consists of three tubes. A small tube of varnished copper (which is fastened into the only perforation which communicates with the cock, and of course with the glass recipient) passes up in the axis of a glass tube (T, fig. 4), open at top, cemented into a socket (S, fig. 4), which screws on to the cock. A smaller glass tube is placed in the interstice between the external glass tube and the copper tube in its axis. This intermediate glass tube is open at its lower termination, but at the upper one is closed or opened at pleasure by a screw. The interstices between the three tubes are partially supplied with water, as represented in the drawing (W, fig. 4). When the passage between the gauge and the recipient is open, if the pressure on the included air be more or less than that of the atmosphere, the water will rise in one of the gauge-tubes, and sink in the other. Other liquids may be substituted for water, in the gauge, when desirable.

In addition to the principal collar of leathers, and screws for rendering that collar compact, there is in the mercurial eudiometers a small hollow cylinder (a piece of a gun-barrel), with an additional collar of cork for confining oil about the rod where it enters the collar of leathers; otherwise, in operating with mercury, the leathers soon become so dry as to permit air or mercury to pass by the rod.

It may be proper to point out, that in operations with the hydro-oxygen eudiometer, accurate measurement is necessary only with respect to one of the gases. In analysing an inflammable gas by oxygen gas, or oxygen by hydrogen gas, it is only necessary that the quantity of the gas which is to be analysed, and the deficit caused by the explosion, should be ascertained with accuracy. The other gas, which must be used in excess, sometimes greater, sometimes less, must, in using the mercurial eudiometer, be made to occupy the gauge. In analysing the air, or any mixture containing oxygen, the gauge is filled with hydrogen gas, as already stated; but, in examining inflammable gas, the atmospheric air may be left in the gauge, as its only active qualities are those of oxygen gas.

Figs. 6 and 7 represent those forms of the sliding-rod eudiometer which I have found most serviceable for experiments with nitric oxide gas; with the solutions of sulphurets; or those of sulphate, or muriate of iron, saturated with nitric oxide.

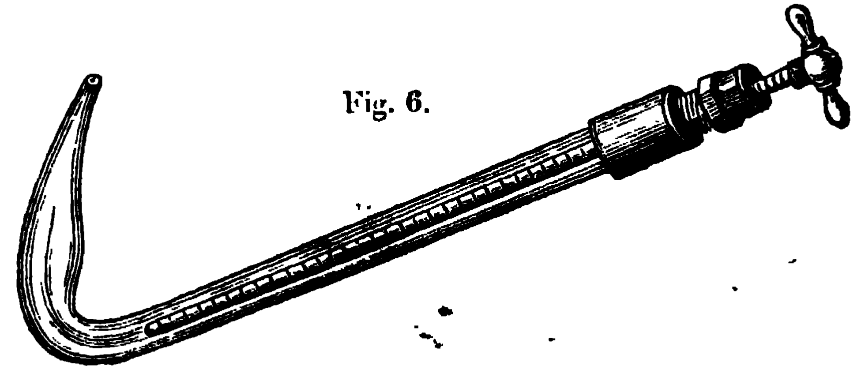


Fig. 6.

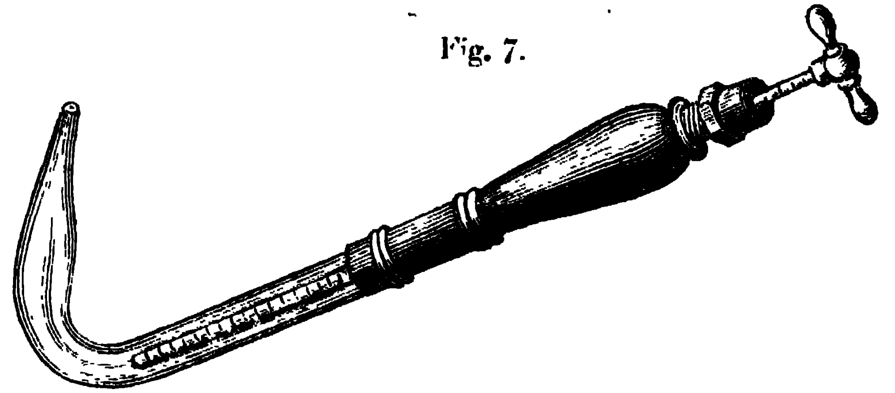
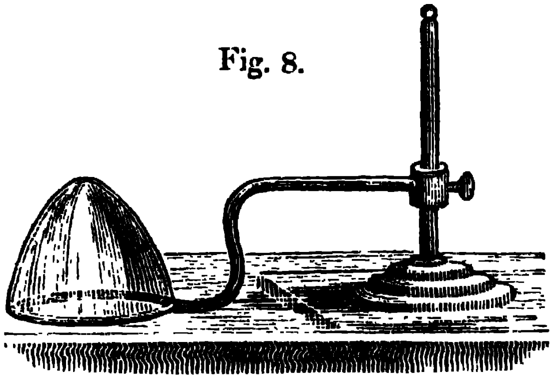


Fig. 7.

The receiver (fig. 8), shaped like the small end of an egg, is employed in these experiments, being mounted so as to slide up and down upon a wire.

This

Fig. 8.



This vessel being filled with water, and immersed in the pneumatic cistern, the apex being just even with the surface of the water, one hundred measures of atmospheric air, and a like quantity of nitric oxide, are to be successively introduced. The residual air may then be drawn into the eudiometer, and ejected again into the receiver through the water, to promote the absorption of the nitrous acid produced. Lastly, it may be measured by drawing it into the instrument, and ejecting it into the egg-shaped receiver (fig. 8), or into the air, when the quantity of it will appear from the number of degrees which the sliding-rod enters during the ejection. That in this way gas may be measured with great accuracy may be demonstrated by transferring any number of measures, taken separately, into the semi-oval receiver, and subsequently remeasuring them.

The eudiometers (figs. 6 and 7), with the accompanying semi-oval glass vessel (fig. 8), may be employed with the dissolved sulphurets, or with solutions of iron, impregnated with nitric oxide in the following way. Let a small phial, with a mouth large enough freely to admit the point of the eudiometer, be filled with the solution to be used. Introduce into the bottle, over the pneumatic cistern, 300 measures of the air or gas to be examined. Transfer the bottle, still inverted, to a small vessel containing water, or a quantity of the absorbing fluid used in the bottle, adequate to cover the mouth of the phial and compensate the absorption. When there has been time enough for the absorption to be completed, transfer the residuum to the receiver (fig. 8), and measure as in the case of nitric oxide.

As soon as I can make a sufficient number of satisfactory observations with the various eudiometers of which I have now given an account, I will send them to you for publication.

III. *On the Theory of the Figure of the Planets contained in the Third Book of the* Mécanique Céleste. *By* J. Ivory, *Esq. M.A. F.R.S.*

[Continued from vol. lxvi. p. 439.]

SOME apology may perhaps be due from me to the readers of the Philosophical Magazine, for drawing their attention to a subject so much neglected in the present times as that which I have undertaken to discuss. It seems to be the general disposition to rest entirely contented with what has already been accomplished in the theory of the figure of the planets. But as all the useful and most important results had already been obtained by Clairaut, we ought, in order to be consistent, to go back to the luminous and elegant theory of that excellent geometer. It will be answered, that the theory in question is imperfect, inasmuch as it merely demonstrates the equilibrium of a planet when it is supposed to have the figure of an oblate spheroid of revolution. The objection is of great weight; and it never can be admitted that the successors of Newton have perfected his system, until the figure of a planet is clearly deduced from the laws of equilibrium without any adventitious supposition. The learned researches of Legendre and Laplace have generally been supposed to obviate the foregoing objection, at least when the bodies are nearly spherical, as is universally true of the planets. But an attentive examination will show that there is still something imperfect in the theory of the illustrious geometers we have named. There is, in fact, involved in it a hidden principle which is equivalent to the gratuitous supposition of Clairaut. The perpendicularity of gravity to the outer surface is common to both; but, as this principle is alone insufficient, Clairaut assumes the figure of an oblate spheroid, while the *analytical method* employed by Legendre and Laplace dispenses with any such supposition in the particular case they have considered. But is it possible that the varying of an abstract method of calculation can in any respect alter the physical foundations of the problem? In order to solve this difficulty, it is to be observed that Legendre and Laplace proceed upon a deficient theory of equilibrium; a necessary condition is omitted; but it so happens that, in the particular circumstances of the problem to which they have confined their attention, the omission may be made without leading to error in a first approximation, and in a first approximation only. This sufficiently explains why a result is obtained that agrees with the solution of Clairaut. But if the result of a first approximation be correct, it is not correctly obtained. In a legitimate investigation we must first know all the conditions of equilibrium: we must then demonstrate that in particular circumstances some of them to a certain extent become unnecessary; and having thus obtained sure principles to proceed upon, we may employ mathematical reasoning and the operations of analysis to complete our purpose. A calculation cannot be unexceptionable, even although it lead to a result not erroneous, when a necessary principle has escaped the penetration of the analyst. We may add, that a theory can never be reduced to the utmost simplicity of which it is capable, unless the physical principles are completely separated from the mathematical processes. These observations will help to explain the purpose intended by the present discussion. It would be ridiculous and a want of common sense to object on slight grounds to any thing sanctioned by the name of Laplace, or to detract from a reputation placed on such solid foundations, and which will always derive part of its lustre from the theory to which our present attention is directed. But in an intricate and difficult investigation, every bay and creek that can possibly lead to error must be explored, before the right track is discovered, and before we can arrive at a successful termination. The researches of Maclaurin and Clairaut were occasioned by the speculations of Newton; the labours of Legendre and Laplace were intended to perfect those of their predecessors; and, if some steps still remain to be made, there is a field fairly open to future inquirers.

It follows from what has been shown in the last number of this Journal, that, in the theory of Laplace, the equation at the surface of the spheroid is always true when the molecules, or small masses of matter on the surface of the sphere, are placed at a distance from the assumed point. In these circumstances the thickness of the molecules may vary in any manner without being subject to the law of continuity. But the equation cannot be true for molecules indefinitely near the assumed point, unless their thickness be restricted to a certain class of functions. In his later writings Laplace, supposing the law of attraction to be as in nature, has limited the equation to the case when the thickness of the stratum near the point of contact of the sphere and spheroid decreases as the square of the distance from that point. With this limitation the equation is no doubt rigorously demonstrated; but we are still left in the same uncertainty as before; since we are not informed what kind of functions is comprehended under the hypothesis assumed. When this point is inquired into, it turns out that the theorem is now too much restricted for the use to be made of it. If we suppose that the thickness of the stratum is a finite and integral function of three rectangular co-ordinates, we embrace all the applications to the figure of the planets; the demonstrations are clear and effected by the usual processes; and near the point of contact the thickness is divisible by the distance from that point, which is much more general than the cases comprehended in the demonstration of Laplace.

By substituting the value of V expanded in a series, in the equation that takes place at the surface of the spheroid, the author of the *Mécanique Céleste* proves that the function y in the value of the radius may always be expressed in a series of terms, each of which is determined by an equation in partial fluxions, to which it is subject. This is a fundamental point in the analytical theory; and as it is a consequence of the equation at the surface, it can be considered as true only in the cases in which that equation is clearly proved. Yet in the whole course of the work the symbol y is considered as perfectly general, and as standing for any function; which cannot fail to embarrass the reader, since the proof of the equation is deficient and limited. Instead of deducing the development in question from the equation at the surface, it will be much more simple to deduce it from the same formula on which the equation itself has been shown to depend: by this means the whole theory will rest upon a single analytical proposition.

Now, resume the second of the formulæ (2),

$$\int (r^2-a^2) \times \frac{(y-y')\,ds}{f^3} = 0,$$

and separate it into the two parts of which it consists, then,

$$y \times \int \frac{(r^2-a^2)\,ds}{f^3} = \int \frac{(r^2-a^2)\,y'\,ds}{f^3}.$$

As f is a function of ψ, if we put $ds = a^2\, d\psi \sin\psi\, d\phi$, it becomes easy to find the integral on the left side of the equation. For the arcs ψ and ϕ are independent of one another; and the integration being effected, first with regard to $d\phi$, between the limits $\phi = 0$ and $\phi = 2\pi$; and then with regard to $d\psi$, between the limits $\psi = 0$ and $\psi = \pi$, the result will be equal to $4\pi a$ when $r = a$. The last equation will therefore become

$$4\pi a y = \int \frac{(r^2-a^2)\,y'\,ds}{f^3};$$

and if we consider y' as a function of the arcs θ' and ϖ', we shall have $ds = a^2\, d\theta' \sin\theta'\, d\varpi'$, and

$$y = \frac{1}{4\pi a} \cdot \int \frac{(r^2-a^2)\, a^2 y'\, d\theta' \sin\theta'\, d\varpi}{f^3}. \qquad (3)$$

Again, for the sake of brevity in writing, let us put

$$\rho = \frac{1}{f} = \frac{1}{\sqrt{r^2 - 2ra\cos\psi + a^2}};$$

then we shall have

$$y = \frac{1}{4\pi a} \cdot \int \left(\rho + 2a\frac{d\rho}{da}\right) . a^2 y' \, d\theta' \sin\theta' \, d\varpi'. \qquad (4)$$

For this latter formula is no more than the first one written differently, as will be manifest by performing the differentiation of ρ with respect to a.

It is to be observed here, that Lagrange conceived that the formula (4) contained the whole of Laplace's demonstration, without its being necessary to add any limitation whatever relating to the thickness of the molecules near the attracted point. For he says explicitly * that the function

$$\rho + 2a\frac{d\rho}{da},$$

is always identically equal to zero on account of the evanescent factor it contains; whence it would follow that the integral

$$\int \left(\rho + 2a\frac{d\rho}{da}\right) a^2 y' \, d\theta' \sin\theta' \, d\varpi'$$

must be equal to nothing, whatever y' stand for, and not equal to $4\pi a y$ as in the formula (3), and as he himself actually found to be true. There is therefore an inconsistency between the reasoning of Lagrange and the result of calculation; and it is this which he calls *une difficulté singulière*, and a *paradox* in the integral calculus. Now all this arises from not observing that the function mentioned is not in every case equal to zero. It is so, indeed, for every point of the surface of the sphere except one, when $\cos\psi = 1$; in which case the function has an evanescent divisor which balances the evanescent factor and produces a finite value. If one element of an integral have a finite value, the integral itself must be a finite quantity; and this is the plain and short solution of the difficulty. If Lagrange's attention had been directed to the formula

$$\int \left(\rho + 2a\frac{d\rho}{da}\right) a^2 (y' - y) \, d\theta' \sin\theta' \, d\varpi';$$

and if he had observed that Laplace limited his theorem to the case when $y' - y$ is divisible by the evanescent factor which appears in the denominator when the molecule is very near the point of contact of the two surfaces, there would have been neither difficulty nor paradox. But although he would in this manner have avoided inconsistency, he would not have obtained the most general demonstration of the theorem. For this purpose we must recollect that the expression we are considering is a double fluent depending on two variable quantities. Let the variable quantities be functions of ψ and φ; then the element of the surface of the sphere will be $d\psi \sin \psi \, d\varphi$, and the expression may be thus written,

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* *Journ. de l'Ecole Polyt.* tom. viii. p. 62.

$$\int \left(\rho + 2a \frac{d\rho}{da}\right) d\psi \sin \psi \int a^2 (y' - y) \, d\varphi.$$

Now, the integral $\int (y' - y) \, d\varphi$ being taken between the limits $\varphi = 0$ and $\phi = 2\pi$, it will be a function of $\cos \psi$; and it is sufficient for the demonstration that this function be divisible by the evanescent divisor. By this procedure the utmost extent possible is given to the theorem; and after all, it will be found that we have obtained nothing but what readily follows from usual rules of analysis.

Let ρ or $\frac{1}{f}$ be expanded into a series, viz.

$$\rho = \frac{1}{f} = \frac{1}{r} + \frac{a}{r^2} \cdot C^{(1)} + \frac{a^2}{r^3} \cdot C^{(2)} + \&c.$$

the symbols $C^{(1)}$, $C^{(2)}$, &c. being functions of $\cos \psi$: then by substituting this series in the function on the right-hand side of the formula (4), that function will become

$$\frac{1}{4\pi} \cdot \left\{ \frac{a}{r} \int y' \, d\theta' \sin \theta' \, d\varpi' + \frac{a^2}{r^2} \cdot 3 \int C^{(1)} y' \, d\theta' \sin \theta' \, d\varpi' + \&c. \right\} \quad (5)$$

and by making $a = r$, we shall obtain

$$y = \frac{1}{4\pi} \times \left\{ \int y' \, d\theta' \sin \theta' \, d\varpi' + 3 \int C^{(1)} y' \, d\theta' \sin \theta' \, d\varpi' + \&c. \right\} \quad (6)$$

Now the series (5) converges when a is less than r; and therefore, even when it goes on *ad infinitum*, it may represent a finite quantity to any degree of approximation. But when $a = r$, the principle of convergency disappears, and no exact notion can be formed of the value of any finite number of the terms. It cannot therefore be said, with any precision of ideas, that such a series, consisting of an infinite number of terms without convergency, will represent any finite quantity. The mind cannot take in the whole series; it must be content with a definite portion of it; and no portion can be considered as equal to the quantity from which the whole is derived. It is only when the series breaks off, and consists of an assignable number of terms, that it can be said to represent a given quantity in the extreme case when $a = r$. Now this happens only when y' belongs to a certain class of functions; namely, when it is a finite and rational function of three rectangular coordinates, which likewise comprehends every case in which the formula (4) is strictly demonstrated. For all such functions the equation (6) is exact, the two sides being identical, and differing from one another in nothing, except in the arrangement of the quantities of which they consist. The equations in partial fluxions to which the terms on the right-hand side are subject, are derived from the expressions $C^{(1)}$, $C^{(2)}$, &c.; and they are such as to determine each term separately when the aggregate of the whole is given.

I have now examined particularly the fundamental points of the analysis of Laplace. Such an examination was required in a theory which in other respects is not unexceptionable. In intricate cases, in which there occur difficulties of different kinds, it seems best to acquire correct notions on one part before we proceed to the other parts. If such discussions (but little calculated to make a brilliant display in the eyes of the public) be ill suited to the prevailing taste of the present times, it must be acknowledged that they are necessary, unless we would entirely neglect a branch of knowledge that has always been reckoned of great value.

But it would be improper to pass on to the second branch of my subject without noticing a demonstration of the equation at the surface of the spheroid, which we owe to M. Poisson[*]. This celebrated mathematician, who has particularly studied this branch of analysis, considers the formulæ (3) and (4); and he proposes to demonstrate their truth, supposing that y' stands for any function whatever of the two arcs θ' and ϖ'. We are not therefore left in any uncertainty about the extent of the proposition to be proved. He observes, that on account of the evanescent factor the element of the integral is equal to zero, in all positions of the molecule, except when it is infinitely near the point of contact of the two surfaces, when the denominator is infinitely small. Now, at the point of contact, we have $y' = y$, $\theta' = \theta$, $\varpi' = \varpi$; wherefore, if we put $\theta' = \theta + h$, $\varpi' = \varpi + k$, we shall obtain the value of the double fluent by extending the integration to infinitely small values, positive or negative, of h and k. But while the arcs θ and ϖ acquire the infinitely small variations h and k, the thickness of the molecule y' may be supposed to remain constant; or, which is the same thing, we may put the equation (3) in this form, viz.

$$y = \frac{y}{4\pi} \cdot \int \frac{(r^2 - a^2)\, a\, y'\, d\theta' \sin\theta'\, d\varpi'}{f^3}.$$

He then finds the value of the integral in the manner he proposed; but as the same value may likewise be found by the ordinary rules, this part of the process adds nothing to the main argument. The force of the demonstration turns entirely on the assertion, that we may integrate on the supposition that the thickness of the molecule remains constant.

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[*] *Journal de l'Ecole Polyt.* tom. xii. p. 145.

To enable us to judge of the validity of this supposition, put $y = y + (y' - y)$ in the formula (3); then

$$y = \frac{y}{4\pi} \cdot \int \frac{(r^2 - a^2)\, a\, d\theta' \sin\theta' \, d\varpi'}{f^3}$$
$$+ \frac{1}{4\pi} \cdot \int \frac{(r^2 - a^2)\, a\, (y' - y)\, d\theta' \sin\theta' \, d\varpi'}{f^3}.$$

Now, the first term in the value of y is what results from M. Poisson's supposition, that the thickness of the molecule remains constant. That supposition therefore virtually admits the equality of the second term to zero. It is very plain that, if the second term be not equal to zero, we shall not obtain the exact value of the double fluent by integrating on the supposition that the thickness of the molecule is constant. Now it is to prove that the second term in the foregoing value of y is evanescent, that Laplace has taken so much pains without having given a satisfactory demonstration of it. It has likewise been shown above, that the evanescence of the same quantity is in reality the foundation of the whole analytical theory. It would be superfluous to add another word respecting M. Poisson's demonstration, which affords no additional evidence of the proposition to be proved.

An attentive reader who considers the foregoing observations must allow that some material inadvertencies and inaccuracies have originally slipt into the analysis of Laplace. But the theory having been published, it has been deemed advisable to repel all objections, and to defend it *to the utterance.*

Jan. 6, 1826. JAMES IVORY.

[To be continued.]

IV. *Sequel of the Memoir of M.* AMPERE *on a new Electro-dynamic Experiment, on its Application to the Formula representing the mutual Action of the two Elements of Voltaic Conductors, and on new Results deduced from that Formula.*

[Concluded from vol. lxvi. p. 387.]

WE have found, in the applications which we have just made of the formula which expresses the mutual action of two infinitely small portions of voltaic conductors, (see page 385 of this memoir in the preceding volume of the Philosophical Magazine)

$$2 \frac{dM}{d\theta} d\theta = -aii' (\cos\theta - \sin\theta) \left(\frac{1}{\sin^2\theta \cos^2\theta} + \frac{1}{\sin\theta\cos\theta} + 1 \right) d\theta$$

for the differential momentum of rotation in virtue of which a rectilinear conductor, of which the length is $2a$, moveable around its centre, oscillates from side to side of its situation of equilibrium, when it is submitted to the action of two fixed conductors, each of which has one of its extremities at this centre, and whose length is a. In the instrument which I have contrived for verifying this result of my formula, it is not only these two conductors which act on that which is moveable, but also the circular portion of the voltaic circuit which joins the two other extremities of the fixed conductors: as the action which results from this portion is exerted in a contrary direction, a momentum is obtained of which the sign is opposed to that of the momentum of which we have just obtained the value, it must be added to the first; and what is very remarkable, the total momentum takes a form much more simple. In short, in naming M' the momentum of rotation produced by this arc, that which must be added to $2\frac{dM}{d\theta}d\theta$

is evidently $$2\frac{dM'}{d\theta}d\theta;$$

as the radius of the arc s' is equal to a, we have $s' = 2a\theta + C$,

whence $$d\theta = \frac{ds'}{2a},$$

and, consequently, $$2\frac{dM'}{d\theta}d\theta = 4a\frac{dM'}{ds'}d\theta.$$

But the tangential force in the direction of the element ds'

being $\frac{1}{2}ii'ds'd\frac{\cos^2\beta}{r}$,

and its momentum of causing this element to turn round its centre being equal, and of a sign contrary to that whose value we are seeking, we have

$$\frac{d^2M'}{ds\,ds'}ds\,ds' = -\tfrac{1}{2}aii'ds'd\frac{\cos^2\beta}{r},$$

whence $$\frac{dM'}{ds'}ds' = -\tfrac{1}{2}aii'\left(\frac{\cos^2\beta''}{r''} - \frac{\cos^2\beta'}{r'}\right)ds'.$$

Observing that it is necessary to integrate in the same manner in relation to the direction of the current as for rectilinear fixed conductors, we find

$$\cos\beta' = -\cos\theta,\ r' = 2a\sin\theta,\ \cos\beta'' = \sin\theta,\ r'' = 2a\cos\theta,$$

thus

$$\frac{dM'}{ds'} = \tfrac{1}{4}ii'\left(\frac{\cos^2\theta}{\sin\theta} - \frac{\sin^2\theta}{\cos\theta}\right) = \tfrac{1}{4}ii'(\cos\theta - \sin\theta)\left(\frac{1}{\sin\theta\cos\theta} + 1\right),$$

and $$4a\frac{dM'}{ds'}d\theta = aii'(\cos\theta - \sin\theta)\left(\frac{1}{\sin\theta\cos\theta} + 1\right)d\theta.$$

Uniting this momentum with that which we have called

$$2\frac{dM}{ds}\cdot d\theta,$$

we have $-\frac{a\,i\,i'\,(\cos\theta - \sin\theta)}{\sin^2\theta\cos^2\theta}\,\mathrm{d}\,\theta = -\frac{4\,a\,i\,i'\sqrt{2}\sin\frac{1}{2}\eta}{\cos^2\eta}\,\mathrm{d}\,\theta$,

because, besides the equation $\sin\theta\cos\theta = \frac{1}{2}\cos\frac{1}{2}\eta$ which we have deduced (page 394 of the former portion of this memoir) from the value of θ, $\theta = \frac{1}{2}\varepsilon = \frac{1}{2}\left(\frac{\pi}{2} - \eta\right)$, we obtain also from this same value

$$\cos\theta - \sin\theta = \sqrt{2}\sin\tfrac{1}{2}\eta.$$

The action which causes the moveable conductor to oscillate is then proportionate to the sine of the quarter of the angle comprised between the directions of the two fixed rectilinear conductors, divided by the square of the cosine of the half of the same angle; it becomes null with this angle, as it ought to be, and infinite when they are directed following the same right line, because then $\eta = \frac{\pi}{2}$.

In the instrument intended for the measurement of these oscillations, the two extremities of the moveable conductor are also joined by a conductor forming a semi-circumference; but account is only to be taken of the action exercised on its rectilinear portion; since the circuit formed by the two fixedre ctilinear conductors, and by the arc which joins the extremities of it, is a closed circle which cannot act on the circular portion of the moveable conductor.

The value which we have found for the elementary momentum

$$\frac{\mathrm{d}\,M'}{\mathrm{d}\,s'}\mathrm{d}\,s' = -\tfrac{1}{2}a\,i\,i'\left\{\frac{\cos^2\beta''}{r} - \frac{\cos^2\beta'}{r}\right\}\mathrm{d}\,s'$$

expresses generally the action impressed by the little arc $\mathrm{d}\,s'$ on a conductor of any form whatever, so as to make it turn round an axis elevated by the centre of this arc perpendicularly to its plane: this action is then independent of the form of this conductor, and only depends on the situation of its two extremities relatively to the little arc $\mathrm{d}\,s'$; it is equal, as it ought to be, to the produce of the radius a by the value which we have obtained (see vol. lxvi. p. 378) for the force which is exercised on the same moveable conductor by a small portion equal to $\mathrm{d}\,s'$ of a rectilinear conductor directed according to this arc $\mathrm{d}\,s'$. When we wish to see the action of an arc terminated, we must integrate afresh with relation to s', and this second integration generally gives a different result in the two cases; but this result is the same when the moveable conductor has one of its extremities in the axis, and the other on the circumference of which the arc s' makes a part. The only sign of the value which is obtained becomes changed, because in one case β augments with s', and diminishes in the other; for then the angle β' is a right angle, and the angle β'' is comprised between a chord and a tangent formed by the extremity, whence it is easy to conclude

$$r = 2a\sin\beta,\ s' = c - 2a\beta,\ \mathrm{d}s' = -2a\,\mathrm{d}\beta,$$

which gives $$\frac{\mathrm{d}s'}{r} = -\frac{\mathrm{d}\beta}{\sin\beta},$$

and for the value of the momentum sought

$$\tfrac{1}{2} a i i' \int \frac{\cos^2\beta\,\mathrm{d}\beta}{\sin\beta},$$

which is precisely the same form as that of the force in the case of the rectilinear conductor, and is integrated precisely in the same manner. The reason of [illegible] analogy between these two cases, otherwise so different, is [illegible] in this circumstance,—that in that of the rectilinear conductor we had

$$r = \frac{a}{\sin\beta},\ s = -a\cot\beta,\ \mathrm{d}s' = \frac{a\,\mathrm{d}\beta}{\sin^2\beta};$$

whence we obtain $$\frac{\mathrm{d}s'}{r} = \frac{\mathrm{d}\beta}{\sin\beta},$$

which differs only by the signs of the value of $\frac{\mathrm{d}s'}{r}$ in the case of the circular conductor; which ought to be so, because in the first, β diminishes when s' augments, and because it augments with s' in the second.

Let us now consider two rectilinear conductors the directions of which form a right angle, but may not be situated in the same plane, by naming a the right line which measures the distance of these directions, and by taking the points where they are met by the right line a for the origin of s and of s', we have

$$r^2 = a^2 + s^2 + s'^2,\ r\frac{\mathrm{d}r}{\mathrm{d}s'}\mathrm{d}s' = s'\mathrm{d}s',$$

and $$\cos\beta = -\frac{\mathrm{d}r}{\mathrm{d}s'} = -\frac{s'}{r}.$$

But we have seen (vol. lxvi. page 381) that the mutual action of the two elements $\mathrm{d}s$ and $\mathrm{d}s'$ is generally equal to

$$\tfrac{1}{2} i i' \frac{\mathrm{d}s'}{\cos\beta}\,\mathrm{d}\frac{\cos^2\beta}{r};$$

it may then be written thus,

$$\tfrac{1}{2} i i' r s'\,\mathrm{d}s'\,\mathrm{d}\frac{1}{r^3};$$

and as this force must be multiplied by $\frac{a}{r}$ to have its component parallel to the right line a, the value of this component is found to be $$-\tfrac{1}{2} a i i' s'\,\mathrm{d}s'\,\mathrm{d}\frac{1}{r^3},$$

by integrating it with relation to s between two points whose distances to the element ds' may be r' and r'', we have

$$-\tfrac{1}{2}\, a\, i\, i'\, s'\, ds' \left(\frac{1}{r''^3} - \frac{1}{r'^3}\right),$$

which may be written thus

$$-\tfrac{1}{2}\, a\, i\, i' \left(\frac{1}{r''^2} \cdot \frac{dr''}{ds'}\, ds' - \frac{1}{r'^2} \cdot \frac{dr'}{ds'}\, ds'\right),$$

of which the integral, taken for the first term of $r_{/}''$ to $r_{//}''$, and for the second of $r_{/}'$ to $r_{//}'$, gives

$$\tfrac{1}{2}\, i\, i' \left(\frac{a}{r_{//}''} - \frac{a}{r_{/}''} - \frac{a}{r_{//}'} + \frac{a}{r_{/}'}\right),$$

so that the action sought is precisely the same as if it were produced by four forces equal to $\frac{1}{2}\, i$ [illegible] according to the right lines [illegible] two by two the extremities of the conductors, two of [illegible] forces being attractive and the other two repulsive.

If there be required the momentum of rotation impressed in the case which we are here examining, by one of the two rectilinear conductors on the other conductor around an axis parallel to the first, [illegible] whose shortest distance to the line which we have named a be represented by b, it will be necessary to multiply the component parallel to a of the mutual action of the two elements by $s' - b$, and then integrate in the same manner: as s' is constant in [illegible] integration, it will suffice to perform this multiplication after it has been executed; thus we shall have two terms of the same form to integrate anew, the first will be

$$-\tfrac{1}{2}\, a\, i\, i' \frac{s'-b}{r''^2} \cdot \frac{dr''}{ds'}\, ds',$$

and there will come, by integrating partially,

$$\tfrac{1}{2}\, a\, i\, i' \frac{s'-b}{r''} - \tfrac{1}{2}\, a\, i\, i' \int \frac{ds'}{r}.$$

But it is easy to see that by naming c the value of s which corresponds to r'', and which is a constant in the actual integration, we have

$$r'' = \frac{\sqrt{a^2+c^2}}{\sin \beta''},\quad s' = -\sqrt{a^2+c^2} \cot \beta'',\quad ds' = \frac{\sqrt{a^2+c^2}}{\sin^2 \beta''}\, d\beta'',$$

thus
$$\int \frac{ds'}{r''} = \int \frac{d\beta''}{\sin \beta'} = 1\, \frac{\operatorname{tang} \frac{1}{2} \beta_{//}''}{\operatorname{tang} \frac{1}{2} \beta_{/}''}$$

the second term will be integrated in the same manner, and we shall have at last, for the momentum of rotation sought,

$$\tfrac{1}{2}\, a\, i\, i' \left(\frac{s_{//}' - b}{r_{//}''} - \frac{s_{/}' - b}{r_{/}''} - \frac{s_{//}' - b}{r_{//}'} + \frac{s_{/}' - b}{r_{/}'} - 1\, \frac{\operatorname{tang} \frac{1}{2} \beta_{//}''\, \operatorname{tang} \frac{1}{2} \beta_{/}'}{\operatorname{tang} \frac{1}{2} \beta_{/}''\, \operatorname{tang} \frac{1}{2} \beta_{//}'}\right).$$

In the case where the axis of rotation parallel to the right line s passes by the point of intersection of the two right lines

a and s', we have $b = 0$; and if we suppose, besides, that the current which flows along s' departs from this point of intersection, we shall moreover have

$$s_{\prime}' = 0,\ \beta_{\prime}' = \frac{\pi}{2},\ \beta_{\prime}'' = \frac{\pi}{2},$$

so that the value of the momentum of rotation will be reduced to

$$\tfrac{1}{2}\, a\, i\, i' \left(\frac{s_{\prime\prime}''}{r_{\prime\prime}''} - \frac{s_{\prime\prime}'}{r_{\prime\prime}'} - \mathrm{l}\, \frac{\operatorname{tang} \frac{1}{2} \beta_{\prime\prime}''}{\operatorname{tang} \frac{1}{2} \beta_{\prime\prime}'} \right).$$

We have just seen that when the directions of the two rectilinear conductors of which we seek the mutual action, form a right angle, that of the two elements of s and s' becomes reduced to

$$- \tfrac{1}{2}\, i\, i'\, r\, s'\, \mathrm{d}\, s'\, \mathrm{d}\, \frac{1}{r^3},$$

and that we have, in the same case,

$$r = \sqrt{a^2 + s^2 + s'^2}:$$

then this elementary action may be thus written,

$$-\tfrac{1}{2}\, i\, i'\, s'\, \mathrm{d}\, s' \sqrt{a^2 + s^2 + s'^2}\, \mathrm{d}\, (a^2 + s^2 + s'^2)^{-\frac{3}{2}}$$

$$= \tfrac{3}{2}\, i\, i' \frac{s\, s'\, \mathrm{d}\, s\, \mathrm{d}\, s'}{(a^2 + s^2 + s'^2)^2}.$$

As it acts in the direction of the right line r, it is necessary, to find the momentum of rotation which results from it around the right line a, to multiply it by the sine of the angle contained between its direction and that of this right line, which is equal to

$$\frac{\sqrt{s^2 + s'^2}}{\sqrt{a^2 + s^2 + s'^2}},$$

and by the shortest distance

$$\frac{s\, s'}{\sqrt{s^2 + s'^2}},$$

that is to say, that the force must be multiplied by the quantity

$$\frac{s\, s'}{\sqrt{a^2 + s^2 + s'^2}},$$

which I shall represent by q, which gives

$$\frac{\mathrm{d}^2 M}{\mathrm{d} s\, \mathrm{d} s'}\, \mathrm{d}\, s\, \mathrm{d}\, s' = \tfrac{3}{2}\, i\, i' \frac{s^2\, s'^2\, \mathrm{d}\, s\, \mathrm{d}\, s'}{(a^2 + s^2 + s'^2)^{\frac{5}{2}}}.$$

This value at first does not appear easy to integrate; but if we distinguish the value of q once with relation to s, and the other by varying s', we have

$$\frac{\mathrm{d}\, q}{\mathrm{d}\, s} = \frac{s'}{\sqrt{a^2 + s^2 + s'^2}} - \frac{s'\, s^2}{(a^2 + s^2 + s'^2)^{\frac{3}{2}}} = \frac{a^2\, s' + s'^3}{(a^2 + s^2 + s'^2)^{\frac{3}{2}}},$$

$$\frac{\mathrm{d}^2 q}{\mathrm{d}\, s\, \mathrm{d}\, s'} = \frac{a^2 + 3 s'^2}{(a^2 + s^2 + s'^2)^{\frac{3}{2}}} - \frac{3 (a^2 + s'^2)\, s'^2}{(a^2 + s^2 + s'^2)^{\frac{5}{2}}} =$$

$$\frac{a^2}{(a^2 + s^2 + s'^2)^{\frac{3}{2}}} + \frac{3 s^2\, s'^2}{(a^2 + s^2 + s'^2)^{\frac{5}{2}}},$$

so that $\frac{d^2 M}{ds\,ds'} ds\,ds' = \frac{1}{2} i i' \left[\frac{d^2 q}{ds\,ds'} ds\,ds' - \frac{a^2\,ds\,ds'}{(a^2+s^2+s'^2)^{\frac{3}{2}}}\right]$;

the quantity $\frac{a^2\,ds\,ds'}{(a^2+s^2+s'^2)^{\frac{3}{2}}}$

integrated first with relation to s, so that the integral becomes null with s, gives

$$\frac{a^2 s\,ds'}{(a^2+s'^2)\sqrt{a^2+s^2+s'^2}},$$

that it remains to integrate by only varying s', the most simple means to come there is to make

$$\sqrt{a^2+s^2+s'^2} = \sqrt{u} - s',$$

which gives

$$s = \frac{u-a^2-s^2}{2\sqrt{u}},\quad \frac{ds'}{\sqrt{a^2+s^2+s'^2}} = \frac{du}{2u},$$

$$a^2 + s'^2 = \frac{(u-a^2-s^2)^2 + 4a^2u}{4u} = \frac{(u+a^2-s^2)^2 + 4a^2s^2}{4u},$$

and changes the quantity to integrate into

$$\frac{\frac{a\,du}{2as}}{1 + \frac{(u+a^2-s^2)^2}{4a^2s^2}},$$

of which the integral, taken so that it vanishes when $s = 0$ is

$$a\left\{\text{arc tang}\,\frac{(s'+\sqrt{a^2+s^2+s'^2})^2+a^2-s^2}{2as} - \text{arc tang}\,\frac{a}{s}\right\},$$

which becomes reduced, by executing the indicated operations and in calculating by the formula known the tangent of the difference of the two arcs, to

$$a \text{ arc tang}\,\frac{ss'}{a\sqrt{a^2+s^2+s'^2}} = a \text{ arc tang}\,\frac{q}{a}.$$

We have then for the value of the momentum M of rotation, in the case where the two electric currents, of which the lengths are s and s', depart from points where their directions meet the right line which measures the shortest distance from it,

$$M = \tfrac{1}{2} i i' \left(q - a \text{ arc tang}\,\frac{q}{a}\right),$$

when $a = 0$, we have evidently $M = \frac{1}{2} i i' q$, that which agrees with the value $M = \frac{1}{2} i i' p$ which we have already found (page 382), because then q becomes the perpendicular which was then distinguished by p. If we suppose a infinite, M becomes null, as it should be, because that in this case a arc tang $\frac{q}{a} = q$.

If we name z the angle of which the tangent is

$$\frac{ss'}{a\sqrt{a^2+s^2+s'^2}},$$

we shall have $$M = \tfrac{1}{2} i i' q \left(1 - \frac{z}{\text{tang}\, z}\right);$$
it is the value of the momentum of rotation which would be produced by a force equal to

$$\tfrac{1}{2} i i' \left(1 - \frac{z}{\text{tang}\, z}\right),$$

acting according to the right line which joins the two extremities of the conductors opposed to those where they are met by the right line which measures the shortest distance of it.

It is, for the rest, easy to see that if, instead of supposing that the two currents depart from the point where they meet the right, we had made the calculations for what limits soever, we should have found a value of M composed of four terms of the form of that which we have obtained in this particular case, two of these terms being positive and two negative.

By combining the last result which we have just obtained with that which we found immediately before, it is easy to calculate the momentum of rotation resulting from the action of a conductor having for its form the perimeter of a rectangle, and acting on a moveable conductor around one of the sides of a rectangle, when the direction of this conductor is perpendicular to the plane of the rectangle, whatever in other respects be its distance from the other sides of the rectangle, and the dimensions of this one. In determining by experiment the instant when the moveable conductor is in equilibrium between the opposed actions of the two rectangles situated in the same plane, but of different sizes and at different distances of the moveable conductor, we have a very simple means of procuring verifications of my formula susceptible of great precision: it is that which we may easily make with the instrument of which I spoke above, by conveniently modifying the fixed conductors which make a part of it.

The same calculations may be made for any value whatsoever of the angle of the directions of the two rectilinear conductors: by naming this angle ε, we have

$$r = \sqrt{a^2 + s^2 + s'^2 - 2ss' \cos \varepsilon},$$

and in always representing by q the quantity $\frac{s s}{r}$, we find that the force parallel to the right line a is equal to

$$\tfrac{1}{2} i i' \left(\frac{a}{r} + a \cos \varepsilon \iint \frac{ds\, ds'}{r^3}\right).$$

The momentum of rotation around the right line a is then equal to $$\tfrac{1}{2} i i' \left(q \sin \varepsilon - r \cot \varepsilon - \frac{a^2}{\sin \varepsilon} \iint \frac{ds\, ds'}{r^3}\right).$$

As to the integral which enters into these expressions

$$\iint \frac{d s \, d s'}{r^3} = \int \frac{(s - s' \cos \iota) \, d s'}{(a^2 + s'^2 \sin^2 \iota) \sqrt{a^2 + s^2 + s'^2 - 2 s s' \cos \iota}},$$

we may obtain by the known method of integration of differentials which comprise a radix of the second order, and more easily by a particular process which I shall explain elsewhere.

V. *On the Theory of Evaporation. By* Thos. Tredgold, *Esq.*

To Mr. R. Taylor.

Sir,

EVAPORATION has been considerably attended to, but rather as a matter of experimental research than with the object of finding those first principles which are essential to the process. In the following inquiry it is not intended to limit it to a particular case, but simply for illustration the vapour is supposed to be from the surface of water.

When the air in contact with water is saturated with vapour, evaporation ceases, or there is an equilibrium between the powers which produce and retard the formation of vapour.

Now conceive a portion of the vapour to be abstracted from the air, then the equilibrium will be destroyed; and all other circumstances being the same, the tendency to restore the equilibrium must be proportional to the quantity of vapour removed from the previously saturated air; for no other circumstance than the weight of vapour in a given portion of air is altered.

But, the equilibrium being destroyed, evaporation commences, and the vapour cannot be formed without a constant supply of heat; therefore, to obtain this supply of heat when there is no other source than the surrounding bodies of the same temperature, the temperature of the surface where the vapour forms must be depressed, in order that heat may flow to it from the adjoining bodies, or parts of the same body; and as the heat required is proportional to the quantity of vapour formed in a given time, the depression of temperature will be proportional to that quantity.

It will also be obvious that the vapour formed will be of the elasticity corresponding to the temperature of the surface producing it, and therefore will correspond to the depressed temperature of the evaporating surface.

Let T be the general temperature, t the temperature of the evaporating surface at its ultimate depression, and w the weight of vapour in grains that would saturate a cubic foot of air at the temperature t. Then, if it be ascertained by experiment that the evaporation per minute, from a surface of one foot, is a when $w = 1$; we have $1 : a :: w : a w$ = the evaporation when the weight of vapour required for saturation, at the temperature t, is w.

Again: Let e be the evaporation in grains that produces a depression of one degree of temperature, then $T - t = \frac{a w}{e}$; or $T = t + \frac{a w}{e}$. This is, however, not strictly accurate, unless the specific heat of bodies be equal at all temperatures.

The weight of a cubic foot of vapour at the temperature 60°, and pressure 30 inches, is 329·4 grains, and if f be any other force, $30 : f :: 329{\cdot}4 : \frac{329{\cdot}4 f}{30} = 10{\cdot}98\, f =$ the weight of a cubic foot of the force f and temperature 60°. And at the temperature t, $\frac{10{\cdot}98 \times 510 f}{450 + t} = \frac{5600 f}{450 + t}$ nearly. That is, the weight of a cubic foot of vapour at the pressure f and temperature t is $\frac{5600 f}{450 + t}$ grains.

The expansion of dry air by saturating it with moisture appears to be equal to the addition of the same volume of vapour, of the force it would have in a vacuum at the same temperature, but both reduced to the same pressure. Therefore, if p be the greater pressure or force, and p' the less, the spaces being inversely as the forces

$$p : p' :: V : V' = \frac{p' V}{p}$$ = the volume of the rarer fluid

corresponding to the greater pressure, consequently $\frac{p' V}{p} + V = V\left(\frac{p' + p}{p}\right)$ = the volume as increased by expansion.

If the air be so rare that its force is less than that of steam of the same temperature, then p' indicates the force of the air; but whenever the elastic force of the air exceeds the force of steam for the same temperature, then p = the force of the air.

When the forces are the same, or $p' = p$, the volume is doubled by expansion.

General Roy's experiments, as far as they go, accord very well with this formula. The comparison of these experiments made by Mr. Daniell is not, however, quite correct. The volume of the air ought to be its volume at the same temperature as the vapour, and not increased after the operation for expansion, as he has done in his Essays, p. 176. An example will render this more clear; and taking Mr. Daniell's case (which is to find the volume of saturated air at 32°, that of dry air at zero being unity), he has, 30 : 30·216 : : 1 : 1·0072, which, added to the expansion = ·07802, gives 1·08522.

The process ought to be 30 : 30·216 : : 1·07802 : 1·08578. In my own comparison I assumed that the air was saturated at zero; and though the formula gives all the numbers a little in excess, they are nearer than those resulting from Mr. Daniell's calculations.

If these principles of the mixture of vapour with air be correct, a cubic foot of dry air, of the temperature t, will be saturated by $\frac{5600\,f}{450+t}$ grains of vapour of the same temperature. Hence, if x be the temperature of the point of deposition, and t the temperature of the evaporating surface, we shall have

$$5600\left(\frac{f}{450+t} - \frac{f'}{450+x}\right) = w; \text{ and}$$

$$5600\,a\left(\frac{f}{450+t} - \frac{f'}{450+x}\right) = \text{E}, \text{ or the evapo-}$$

tion from a surface one foot square in grains per minute.

As t is only the temperature of the evaporating surface, the general temperature will be $\text{T} = t + \frac{\text{E}}{e}$.

The dynamical question respecting the velocity with which vapour will rise from the evaporating surface remains to be considered, and will most likely give employment to some of your readers.

Thomas Tredgold.

P.S. My thanks are due to Candour for his references to the preceding corrections of Dr. Ure's results: I had overlooked them in the one Journal, and the other I do not regularly see.

VI. *Reply to the Remarks of* Mr. Riddle *on the Double Altitude Problem.* By James Burns, *Esq.*[*]

To the Editor of the Philosophical Magazine and Journal.

Sir,

Mr. Riddle in his concluding remarks on my solutions of the problem of double altitudes, takes it for granted that "we are perfectly agreed," though there is not a single syllable in my communication (nor has he furnished a single proof) that should induce him to think so. I have there asserted that Mr. R. misunderstood the foundation of my method; and I think he has proved that he knew nothing further of the demonstration, which he attempted to advocate, than the mechanical computations derived from it. I shall now, therefore, notice more in detail those solutions of the problem to which I objected, than I had originally intended. The equations which I first gave are,

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[*] [We had hoped that this controversy would have been concluded in our preceding volume, and shall be well pleased if our correspondents will now allow it to terminate.—Edit.]

$$\cos.\ \lambda = \frac{\cos.\ \tfrac{1}{2}(A + a)\ .\ \sin.\ \tfrac{1}{2}(A - a)}{\sin.\ (\tau + \tfrac{1}{2} i)\ .\ \sin.\ \tfrac{1}{2} i\ .\ \cos.\ \delta} \qquad (1)$$

$$\cos.\ \lambda = \frac{\cos.\ \tfrac{1}{2}(A + a)\ .\ \sin.\ \tfrac{1}{2}(A - a)}{\sin.\ \tfrac{1}{2}(T + \tau)\ \sin.\ \tfrac{1}{2}(T - \tau)\ .\ \cos.\ \delta} \qquad (2)$$

Now the second of these is identical with

$$\cos.\ \lambda = \frac{\cos.\ \tfrac{1}{2}(A + a)\ .\ \sin.\ \tfrac{1}{2}(A - a)}{\sin.\ \tfrac{1}{2} i\ .\ \sin.\ (\tfrac{1}{2} i - \tau)\ .\ \cos.\ \delta} \qquad (3)$$

since, $T + \tau = i$; $\tfrac{1}{2}(T + \tau) = \tfrac{1}{2} i$; also, $T - \tau = i - 2\tau$, &c. The only unknown quantity in the equations (1) and (3) is τ, * or the time nearest noon: if that could be *truly* determined, the question could be rigorously and easily solved. But it is plain that τ cannot be so determined by means of the middle time (as in Douwe's method), which is itself determined by means of the latitude by account,—a quantity that may be very far from the truth. Hence the method of Douwe's is no other than a pure paralogism. The more probable way, therefore, of arriving near the truth, would be to take from the "Horary Tables" the angle corresponding to the latitude by account, the greater altitude, and the declination, and substitute it, in one of the above formulæ according to the case; and if the greater altitude were near the meridian, the probable error would be diminished. Mr. R. will *now* probably understand what is meant by "all that is necessary to be known is, the time, the interval, and the altitudes," which before appeared to him so inexplicable.—Now, Dr. Brinkley's method is professedly a correction of the *latitude computed* by Douwe's method, which, by the by, will be often further from the true latitude, than that by account. Let us now see how this correction is derived. The Doctor first deduces the fundamental equation (see Nautical Almanac)

$d c = \frac{d l\ (\text{vers.}\ t - \sin.\ t\ .\ \tan.\ m)}{1 - \tan.\ D\ .\ \cot.\ l}$; or, $d l : d c :: n : 1$, making $n = \frac{1 - \tan.\ D\ .\ \cot.\ l}{\text{vers.}\ t - \sin.\ t\ .\ \tan.\ m}$. The quantity n, therefore (on which the whole demonstration hinges) is a *function* of the latitude by account, of the time nearest noon, determined by means of that latitude and of the middle time connected with it; and must evidently partake, in any future combination, of the inexactness to which each of these quantities may be subject; and that inexactness, we have seen, may be very considerable. We ask then, how is it possible that any combination or transformation of n can lead to an exact result, or to the correction of an inexact one? But to prop this, it is gratuitously supposed that, $t - r : t - c :: n : 1$,

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* The time τ could be rigorously deduced by means of a third altitude and preceding interval; but that being a distinct problem, may be considered on another occasion.

$$\text{Or, } t - c : c - r :: 1 : n - 1,$$

$$\text{Or, } t - c = \frac{c - r}{n - 1}.$$

Now, this implicitly supposes that a certain fixed relation *must always* subsist between t, r, and c, and that they will constantly bear the same invariable relation to n. With such an order of latitudes, the correction certainly may sometimes succeed; but is such order to be always expected in practice? We may with as much truth suppose,

$$r - t : t - c :: n : 1,$$

$$\text{Or } t - c : r + c - 2t :: 1 : n - 1,$$

$$\text{Or } t - c = \frac{r + c - 2t}{n - 1},$$

Or $t = \frac{r + nc}{n + 1}$*; which would considerably change the Doctor's final equations, $t = c + \frac{c - r}{n - 1}$, &c. &c.

It is evident, therefore, that the correction derived from the Doctor's reasoning will be *conditionally* true, and at best but very uncertain in practice. Hence I am not at all surprised that this mode of correction has imposed on Mr. R. Even Douwe's solution, simple as it is, seems to have presented stumbling blocks, which he has not been able to get over. In his first paper, explaining what he calls the times A.M. and P.M., he says "they are not *intended* to represent the true apparent times of observation, but to determine the elapsed interval!—and to find with the aid of the estimated longitude the approximate Greenwich time for determining the declination." Now, without meaning any disrespect, was it possible he did not know that the times A.M. and P.M. do really represent the true apparent times, not in the latitude sought, but in the latitude by account;—that a chronometer has been generally the *only* means used to determine the interval;—and that the declination must enter the computation *before* the times A.M. and P.M. could be determined? From this we might have said at first, *Ex uno disce omnes;* but we were willing to hear all that Mr. R. could say. In his last remarks is given a curious explanation of the assertion, "He assumes as known, not only the interval of time between the observations, *but the true apparent time at each observation;*" for it is said, "I noted the assumption in *italics.*" Now we are to understand from this, henceforth, that "noting a passage in italics" must clear one of the charge of misconception or misconstruction! My having changed the order of the words does not make the least change in the sense of the passage certainly. In Mr. R.'s last paragraph, where he says, "I failed in giving *any* solution of the problem," his language is not only inaccurate but uncandid; for only one method had been proposed when his first remarks appeared; and hence the phrase "any solution" is inapplicable: and before his last, two other solutions had been given. The first of these latter, however, Mr. R. does not seem to approve of, though originally proposed by no less an astronomer than Lalande; and the second he passes over in silence, without a single word, for reasons best known to himself. And to show Mr. R. that our resources are not so confined as he imagined, we shall now present him with a *fourth* solution, with an example calculated at full length, lest he may doubt the truth of the formula itself. It is, perhaps, the most convenient of the four, and shorter by nearly half than Dr. Brinkley's correction alone. The four following equations very simply and briefly solve the problem. As I have not seen the method which Mr. R. has *deduced* from Mr. Ivory's investigations, I cannot judge whether it is "the simplest solution of this useful problem that has yet been given."

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* Hence would arise some curious paradoxes; as when $n = 0$, $t = r$; and if $n = 1$, $t = \frac{r + c}{2}$, &c.

Let first polar distance $= a$
second ditto . . $= b$
first zenith distance $= z$
second ditto . . $= z'$
interval . . . . $= m$

The rest as in the third method, page 345. Then,

$$\text{vers. } c = \sin^2 a \,.\, \text{vers. } m \qquad (1)$$

$$\sin. \mathrm{A} = \frac{\sin. b \,.\, \sin. m}{\sin. c} \qquad (2)$$

$$\sin^2 \frac{\mathrm{B}}{2} = \frac{\sin. \frac{1}{2}(z + z' - c) \,.\, \sin. \frac{1}{2}(z' - z + c)}{\sin. c \,.\, \sin. z} \qquad (3)$$

$$\text{vers. } y = \sin. a \,.\, \sin. z \,.\, \text{vers. C} + \text{vers. } (a - z) \qquad (4)$$

Example.—Let $a = 76^\circ\ 0'\ 0''$
$b = 76\ \ 1\ 20$
$z = 48\ 26\ 45$
$z' = 39\ 58\ 45$
$m = 22\ 30\ \ 0$

2 log. sin. a . . . 19·97380
vers. 22°30′ . . . 8·88150

21° 49′ ½ = vers. c = 8·85530

sin. m . . . . . . 9·58284
sin. b . . . . . . 9·98694
ar. co. sin. c . . . 0.42972

A = 87° 16′ sin. = 9·99950

sin. $\frac{1}{2}(z + z' - c)$. 9·73959
sin. $\frac{1}{2}(z' - z + c)$. 9·06589
ar. co. sin. c . . . 0·42972
ar. co. sin. z . . . 0·12591

2)19·36111

$\frac{B}{2} = 28^\circ\ 38'$ sin. = 9·68055

2

57 16 = B
87 16 = A

30 0 = C . . vers. 9·12702
sin. a 9·98690
sin. z 9·87409 11342 nat. vers. $(a - z)$

8·98801 . . log. 9727

$y = 37^\circ\ 53'$ nat. vers. = 21069
∴ lat. = 52 7.

The demonstrations of the third and fourth methods (which methods, I believe, have been given for the first time) we must, for the sake of brevity, omit for the present; but they cannot create any difficulty to those who understand the principles of spherical trigonometry, as delivered in Woodhouse's or Legendre's treatises. We must notice, however, that the change in declination is not considered in the first equation given above, which will seldom make a difference of more than 1′ on the final result. The latitude deduced by the third method, which is rigorously exact, is 52° 5′ 20″.

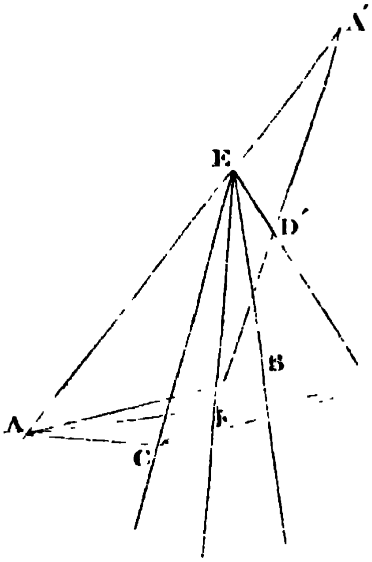
Thus the *candour* and *truth* of Mr. Riddle's statements are apparent. I cannot but acknowledge, however, that there is some wit in his concluding paragraph; but wit is a poor substitute for argument. Yet it is, perhaps, the best resource in the absence of the latter, as it frequently makes a man *appear*, on quitting the field, equal, though seldom superior to his adversary. I remain, sir, your obedient servant,

Gloucester Place, Hackney Road, JAMES BURNS.
January 4, 1826.

ERRATA in the formulæ, page 345 : *For* sin. $\frac{c}{2}$, *read* sin.$^2 \frac{c}{2}$; and *for* sin. $\frac{1}{2} y$, *read* sin.$^2 \frac{1}{2} y$.

VII *Demonstration of Mr.* LEVY'S *Property of the regular Octahedron;—with a Postscript on* P. Q's *Defence of Mr.* HERAPATH'S *Demonstration.* *By* T. S. DAVIES, *Esq.*

THIS very neat but simple theorem was given by its discoverer (unaccompanied however by the demonstration) to Mr. Brooke. The latter gentleman's proof (Crystallography, pp. 317, 318) is unnecessarily complicated; and is, besides, effected by means not strictly mathematical. The following one, it is presumed, is liable to neither of these objections.



Theorem.—Let ABCD be a plane cutting off one of the solid angles E of a regular octahedron; then

$$\frac{1}{AE} + \frac{1}{DE} = \frac{1}{EB} + \frac{1}{EC}.$$

Demonstration.—We assume the truth of the following well-known elementary properties:

1. The diagonals AD, BC of the plane of section intersect in some point F in that diameter of the octahedron which passes through E

2. The angles AED, BEC are right angles.

3. The line EF bisects these right angles.

Then, if $\angle$ EDA $= \varphi$, we have, by trigonometry,

$$\frac{EF}{DE} = \frac{\sin\varphi}{\sin\overline{45^\circ \pm \varphi}}, \text{ and } \frac{EF}{EA} = \frac{\pm\cos\varphi}{\sin\overline{45^\circ \pm \varphi}};$$

whence $$\frac{EF}{DE} + \frac{EF}{AE} = \frac{\sin\varphi \pm \cos\varphi}{\sin\overline{45^\circ \pm \varphi}} = \sqrt{2}$$

the lower sign, " –," referring to the position D'A'.

In a similar manner we find $\frac{EF}{EB} + \frac{EF}{EC} = \sqrt{2}$; and therefore, dividing by EF we get

$$\frac{1}{AE} + \frac{1}{DE} = \frac{1}{EB} + \frac{1}{EC}. \qquad \text{Q. E. D.}$$

Cor. $AE + ED : EB + EC :: AE . ED : EB . EC.$

Postscript on P. Q.'s *Second Defence of* "*Mr.* HERAPATH'S *Demonstration.*"—(*Phil. Mag. vol.* lxvi. *p.* 354.)

I cannot close this short paper without rectifying a slight mistake into which your learned correspondent P. Q. has fallen respecting one or two points in my last communication.

In the first place, I did not "*abandon*" the arguments employed in my first paper on Mr. Herapath's demonstration. They still remain opposed to the view which I *then* took of the process in question: and my second paper was intended to show the inefficiency of that demonstration, *also* under P. Q.'s interpretation of it; and to prove that under "*either* view the same fallacy was involved, the same gratuitous assumption employed." It could only be by an oversight that P. Q. could call my second paper an abandonment of the principles of the first. They are totally *distinct arguments*, and are directed against the two *distinct views* which I conceive may be taken of Mr. Herapath's meaning.

Secondly, the objection to my magical "comparison between the independence of r and v, and that of an angle and its complement" appears also to have been too hastily made. For the addition of an indeterminate number of units to the *fraction* in Mr. Herapath's demonstration is exactly similar to the addition of an *indeterminate number of circumferences* to any fractional portion of a circumference. The truth is, that the inquiry does not call for the consideration of indeterminate *integers:* these may be dropped; and the question would be stripped of its ambiguity by the adoption of two *proper fractions* as the values of r and v. If, however, the indeterminate integers be still contended for, I must still submit that an indeterminate number of circumferences will afford a complete parallel. As subjects of analytical investigation they are of precisely the same character.

I own I was surprised to see so much confidence placed in the argument of P. Q. to establish the *triple condition* of Mr. Herapath's equation, p. 354. When $r + v = n =$ indeterminate integer [$r =$ const.], it cannot be for a moment disputed that $\Delta v = \Delta n$. But are we therefore to admit that gentleman's interpretation of the consequences which flow from this admission? Does not P. Q. perceive, so long as n varies by *integer values only, that* v *varies through a system of fractions whose common difference is an integer, and is altogether incapable of any other system of values whatever?*

Does it need to be urged

$$\text{whilst } \left\{ \begin{array}{l} \Delta v = \Delta n, \text{ and} \\ \Delta n = \text{integer} \end{array} \right\} \text{ that } \Delta v \text{ is also } = \text{integer?}$$

Take, *for "example,"* $r = \frac{1}{2}$, and $n =$ integer: then $v = n - \frac{1}{2}$; and so long as n retains its integral character, v can *never* become $n' \pm \frac{3}{4}$, nor $n'' \pm \sqrt{-1}$. The only system of values which it admits, is comprised in the expression $m \pm \frac{1}{2}$ [n', n'' and m being integers]; and so of other values of r. These "independent variables" are therefore *mutually dependent* during their variation! Thus I have shown by an example, which I thought too simple to need particularly instancing in my last paper, the fallacy of one of those principles which *precede the application* of P. Q.'s very elegant functional theorem, and have therefore completely overturned the ingenious structure raised upon that principle.

It will be recollected that I made no objection to the reasoning in Mr. Herapath's *subsequent* equations, so long as r and v were really independent variables; but wished to show that as r and v were *not* independent variables in the case before us, the conclusions derived on the assumption of that non-existing independence were inadmissible as a demonstration of the binomial theorem. By tracing the process and finding that the independence of r and v was *essential* to the truth of those subsequent equations, I conceive that a complete neutralization was given to the evidence so obtained.

The passage alluded to by P. Q. in his last paragraph certainly was intended as an objection to Mr. Herapath's mode of establishing some theorems in periodical functions, where the indices of the characteristic were *fractional*, that mode being founded on the assumed independence of two fractions whose sum is an integer. I have not the Number at hand, nor had I then; but I think I can depend upon my memory respecting the method. If I erred, let this circumstance apologize for me: but if I have not mistaken the method, and the preceding reasoning be admitted, the fallacy of such a method is apparent.

I can assure Mr. Herapath (in conclusion of a reply which has expanded much further than I intended when I sat down to write), that were I convinced of the accuracy of his method I should "not be backward to acknowledge it." I trust I shall ever feel too sincere a regard for truth to contend upon any question merely for the sake of victory, and too candid to hesitate a single moment in expressing my conviction, whatever may have been my previous opinions, or however my credit may seem to be pledged in their support.

Bath, Dec. 5, 1825.

VIII. *On the Comet of* 1825. *By* THOMAS SQUIRE, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

WITHOUT entering into the nature of those chaotic compounds of elementary substances, or rather incipient worlds called comets, of which we, perhaps, are less acquainted than with their motions; yet, nevertheless, I think it may truly be said that no part of astronomy is more in its infancy than that which relates to the eccentric and anomalous motions of these erratic bodies, which are occasionally and at very uncertain periods observed to visit the bounds of our solar system, when passing through the perihelion parts of their orbits.

Should you, Mr. Editor, think the following computations and remarks, which relate to the comet of 1825 (that first appeared about the beginning of September), entitled to a place in your scientific Journal, they are truly at your service.

On the supposition of a parabolic orbit, this comet must have passed from the northern to the southern side of the ecliptic about the 22d of August; but it was not visible to the naked eye until the 7th of September, when it was seen in the constellation *Taurus*, near *Aldebaran* and the *Hyades;* at which time its distance from the sun was 1·871, and from the earth 1·407. On the 12th of the same month at 1 A.M. its anomaly was 69° 31′ 38″, its distance from the sun 1·8229, and from the earth 1·2391, having also a geocentric longitude of 60° 40′ 19″, and a southern latitude of 6° 34′ 29″. Again, on the 17th, the comet's distance from the sun was 1·767, and from the earth 1·105. It continued thus to approach the earth in a lateral direction till the 12th of October, when by computation it appears to have come nearest to the earth, at which time it was a very conspicuous object in the heavens; when, at midnight, its distance from the sun was 1·525756, and from the earth only ·61471: its geocentric longitude was 35° 8′ 11″, and latitude 35° 51′ 35″ south. Hence it was then in the southern part of the constellation *Cetus*. Therefore at this time it must have been vertical between the parallels of 20 and 21 degrees south, a little before two o'clock that morning, according to the respective meridians. From this it is clear that the comet must have been a very striking object to all the known parts of the southern hemisphere and the low northern latitudes. After the 12th of October the earth and comet gradually receded from each other, so that on or about the 17th of November the comet must have been too far from the earth to be visible, even under the most favourable circumstances of southern latitude. Although the relative motions of the earth and comet were now such as rapidly to increase their lineal distance, yet the comet continued to approach the sun till the 11th of December, when it passed its perihelion point at a distance of 1·2295 from that body.

The earth and comet will continue to recede from each other till about the [illegible] of January; and as the heliocentric motion of the latter [illegible] is retrogra[illegible] being at the same time in an opposite part of the heavens in respect to the earth, the two bodies will for some time move nearly parallel to each other, and towards the same infinite distant point in space, when the comet's distance from the sun will be 1·4, and from the earth 2·28, the latter distance being equal to 21660 millions of miles. Though the orbicular motion of the comet will now carry it rapidly from the sun, yet it will again gradually approach the earth, or more properly, the earth may be said in the race to gain upon the comet till about the 22d of April; and on that day, at $5^h\ 49^m\ 12^s$ M.T. its distance from the sun will be 2·27056, and from the earth 1·37183, having at the same time a geocentric longitude of 243° 49′ 46″, and a southern latitude of 15° 27′ 56″; hence it will be near the star ϑ in the neck of the constellation *Lupus*; at which time, and for a few days before and after, it may again be expected to be visible to the southern parts of the world, but its altitude above our horizon will be too small for it to be seen from our northern position; and by the beginning of May it will be too far from the sun and from the earth to admit of its being any longer visible to the inhabitants of our globe. On the second appearance of this comet it will, properly speaking, be divested of its tail; in which case the nucleus will only be surrounded by a nebulous light.

Yours respectfully,

Epping, Jan. 1, 1826. THOMAS SQUIRE.

P.S. It is a little remarkable that the comet of 1823 passed its perihelion about the same time in December as that of 1825; but the former when in that point of its orbit was nearly at the same distance from the sun, as the latter was beyond the sphere of the earth's orbit, their relative perihelion distances being ·228944 and 1·22950 respectively.

IX. On

IX. *On the Planet Saturn.* *By* M. Smith, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

OBSERVING in a very excellent work just published on telescopes, by Dr. Kitchener, an account of a singular appearance which the planet Saturn presented in the years 1805 and 1818 (for which appearance no reason has been assigned), and conceiving that the phænomenon admits of an easy explanation, I beg leave to trouble you with the following remarks on it.

The passage in Dr. Kitchener's book to which I allude is the following, at page 349.

"The singular figure of which the body of Saturn was observed by Sir William Herschel on April 19, 1805, when he says 'the figure of Saturn is somewhat like a parallelogram, with the four corners rounded off deeply, but not so much as to bring it to a spheroid,' is very like the appearance which the planet presented in September 1818, when I made a sketch of it, which is like to Sir W. H.'s. I have occasionally observed this planet for nearly 30 years, and I do not remember to have seen the body of it of this singular form, except for a few months at the time I have mentioned."

Now, sir, if we consider that in the year 1818 the earth was in the plane of Saturn's equator, and that it is only in that plane once in fifteen years, we shall easily comprehend the reason of this phænomenon. The true figure of Saturn can never be observed except on such occasions, because it is only then that the visible disc of the planet is bounded by a meridian; for it is evident, that whatever be the true figure of the planet (provided it be a solid of revolution), it must to an eye placed vertically over its pole appear a perfect sphere; consequently, as we recede from the plane of its equator it must approximate to the spherical figure:—on this principle we may expect to see the planet again in its true shape in the year 1833. It may here be proper to remark, that when we are in the plane of Saturn's equator we are also in the plane of his ring; and therefore that in making a diagram of the planet it would be improper to draw it of its true shape, except when the ring is represented edgewise, or as a straight line bisecting the body of the planet; for when the ring appears open, the figure of the planet will not sensibly vary from a sphere.

The manner in which the ring of Saturn is balanced, so that the planet shall always occupy its centre, has been thought wonderful even by some celebrated astronomers. To me it appears the simplest thing imaginable; for I think it self-evident that if the ring were removed to a distance of two or three millions of miles from the planet and left at liberty, it must by its own gravity fall towards the planet; and after perhaps impinging thereon, it must continue to fall until its centre of gravity coincides with that of the planet: in which case the planet must of course occupy its centre. Now, if Saturn were a sphere, the ring might assume any accidental position with respect to the equator of the planet; but by reason of the spheroidal figure of Saturn occasioning an excess of gravity towards its equatorial regions, the plane of the ring must be drawn into the plane of Saturn's equator, which is exactly the situation in which we find it: the rotation of the ring on its axis is, therefore, unnecessary to its support.

A very curious subject for speculation, which does not appear to have hitherto suggested itself to the inquiry of astronomers, is the following: What is the use of this stupendous ring, which for extent of surface and solidity of structure (as we may infer from its superior brightness) surpasses even the planet itself? Can it be a habitable world? Certainly it may; for the velocity with which the ring revolves on its axis may be so adjusted as to produce a centrifugal force which shall be an exact counterpoise to the force of gravity towards the planet: and in such case the surface of the ring must appear to the annularians as a horizontal plane; while the body of the planet is seen in the distance like an immense mountain, behind which the sun disappears for about one or two hours (according to circumstances) out of every ten hours, or one revolution of the ring. The edg[illegible] of the ring are probably rounded off, although our instruments will not enable us to verify this fact by observation; and in such case the annularians may travel either by land or water from one surface of the ring to the other without observing any remarkable appearance, except that on passing round the edge of the ring the heavenly bodies will change their altitudes rapidly within a comparatively small space. To those who may be on the inner edge of the ring the body of the planet probably appears as a circular plane directly over their heads, and supported by two great pillars rising from opposite points of the horizon. The satellites of Saturn are probably never seen by the annularians, except by those who may be near the outer edge of the ring; for as they revolve in the plane of the ring, they are always in the horizon: the seventh satellite is, perhaps, an exception; for as it deviates from the plane of the ring, it may occasionally appear a few degrees above the horizon.

It has been conjectured by some who have thought but slightly on the subject, that the ring was constructed for the purpose of enlightening the planet in the absence of the sun. To those who advance this opinion it may be replied, that for the purpose of illumination the ring is worse than useless, inasmuch as that it intercepts more of the sun's light from the planet than it reflects towards it. To exemplify this, let us assume any particular spot on the surface of Saturn. Suppose a spot whose latitude is equal to that of London. Now by duly considering that the plane of the ring is inclined thirty degrees to the plane of Saturn's orbit, it will be perfectly evident, that to the assumed spot the ring can only appear enlightened by the sun during one half of the year, and that the summer half; to which may be added, that all the portion of the ring which at midnight is near the meridian, must be eclipsed by the body of the planet. The phænomena actually observed will therefore be as follows; viz. Immediately after sunset an arm of the ring will appear in the west, which will gradually shorten and finally set; but before it entirely disappears, another similar arm will rise in the east, and gradually lengthen until the superior brilliance of the ascending sun supersedes its use as an object of illumination. About the period of the summer solstice these two arms of the ring will unite so as to form an entire arch intersecting the horizon in the east and west, and inclined thereto at an angle equal to the co-latitude of the place, at which time there will certainly by considerable illumination. Still it may be remarked that the illumination is most perfect when least wanted. This therefore, as well as the fact that the planet is furnished with seven moons, is demonstrative proof that the ring was not constructed for the purpose of illumination; and no other supposition remains than that it was formed to be a habitable world. It may further be remarked, that although the ring cannot usefully enlighten the planet, yet the planet reflects a very strong light on the ring for about half of each period of ten hours; and therefore the annularians have no reason to regret that the satellites do not rise above their horizon, because the planet reflects them, perhaps, ten times more light than would be the united effect of all the satellites.

The ring of Saturn is now known to be double, or to be in fact two concentric rings; but this circumstance does not affect the justness of any of the foregoing arguments. Perhaps this division may be advantageous to the inhabitants, as affording them a short cut from one surface of it to the opposite; or perhaps the adjustment of centrifugal force before alluded to, may require that the velocity of rotation should in a small degree differ in the two rings, in order to produce an equilibrium, or counterpoise to the force of gravity towards the planet; for unless this equilibrium be effected, the surface of the ring could not appear to the inhabitants perfectly horizontal.

It has been remarked by Sir William Herschel, that "the ring of Saturn reflects more light than the body of the planet." The natural inference is, that it is formed of materials of greater specific density; and it seems advantageous that it should be so: for otherwise, on account of its comparative thinness, it could not produce an adequate force of gravity perpendicular to its surface, which we must suppose essential to its being inhabited.

The annularians in their systems of geography can only estimate their latitude by the observed altitude of Saturn's pole; for the sun and all the other heavenly bodies have the same altitude viewed from every part of the flat surface of the ring. As for their longitude, I have not hitherto been able to decide how they ascertain it.

Should the foregoing remarks be thought to merit a place in your Journal, the insertion will much oblige, sir,

Your most obedient servant,

Nov. 17, 1825. M. Smith.

X. *Notices respecting New Books.*

The English Flora, Vol. III. *By Sir* J. E. Smith, *M.D.F.R.S. President of the Linn. Soc., &c. &c. &c.,* 1825.

THERE is a knowledge acquired by practice and experience, which carries us much further into an acquaintance with sensible objects than the best instruction and information can do. This is a familiar observation when applied to such occupations as have to do with an article of trade. The farmer for instance, besides the obvious practice of his business, has a great deal of knowledge, the result of long experience, which is incapable of being communicated, even if his vocabulary were richer than it is; and he could no more acquaint a pupil with all the rules by which he judges of the goodness of his samples of grain, than he could convey to him by words an idea of the looks and expressions by which he knows his neighbour's countenance. The same thing is seen in other occupations. We have been surprised at the dexterity with which a wool-sorter selects from a pack containing different samples, at a single glimpse, the locks of wool of the same quality, while to our unpractised eye there was little or no difference among them. It is this empirical knowledge which gives the practical tradesman such advantage, and far outweighs the superior intellect and acquirements which a theoretical competitor may have. The truth seems to be, that sensible objects have many characters which make so slight an impression on the mind, that they do not in passing through it become the subjects of examination. They are to the eye and to the touch what the various flavours are to the taste,—too delicate and evanescent to be detected and examined as they pass. Hence the nicer qualities of things are long before they are observed, and it is not till they are observed with attention that terms are invented to express them. Here then is an impediment to the progress of knowledge, when no words are capable of expressing the character of an object in consequence of its transient nature; and it is an impediment not likely to be overcome by the practisers of art, but must be left to such as are habituated to watch their own impressions and practised in arresting them.

But the reader will begin to say, how does all this lead to a notice of the English Flora? We come now to the application of our remarks. It cannot but have struck even the unbotanical observer, how much more difficult the science of botany has become by the vast multiplication of species, and by the minute differences which are relied on as sufficient to afford a character. Among European plants, indeed, the science has been followed up with such analytic severity, that naturalists have, in many instances, resorted to the empirical characters which experience has pointed out, but which are either untechnical, and hence cannot be employed in a specific description, or are of such a nature that the mind, though it acts upon the impression, cannot discover it so as to describe it to another. Thus they speak of one species differing from another in habit, appearance, touch, &c.; by which they oftentimes mean that it has some undescribable peculiarities about it, which point it out to a practised observer as distinct. The astutest botanists of the age are all running into this extreme minuteness of distinction; and it can only be explained, we think, by attributing it to the cause we have assigned. It is no reflection upon them that there should be this tendency. On the contrary, it is to their honour that they have carried the analysis as far as their present technical language will assist them. The botany of the old herbalists was, from the want of this language, almost entirely empirical; and we are fast losing ourselves in the same difficulty. In order to be rescued, some new Linnæus must spring up, who shall be possessed of a mind for seizing hold of and describing these subtile characters; and thus we shall artificially be carried on another stage: but whatever

ever depends upon language for its communication and extension must have its bounds.

To illustrate our subject, we refer the reader to Weihe and Nee's *Rubi Germanici*, where he will find the descriptions carried to a minuteness which could only have been produced by the most laborious investigation; and yet, after all (with the exception of a few well-known species), this minute detail does not enable the reader to make out the plant, even with the aid of well executed figures (which mode of representation delineates some of the characters of natural objects far better than words); and in most instances we gain no more information than this,—that the authors saw something different which they are unable to describe. The English have not been behind their neighbours the Germans in the scrutiny to which they have subjected some genera. Take for instance *Juncus*, *Rosa*, *Myosotis*, *Saxifraga*, with some scores of species in other genera. How many of the new ones are purely empirical! In many instances no doubt the distinction is perceived, but it is so minute and fluctuating that it is impossible to reduce it to a specific character, and seldom can be intrusted even to general description.

If any one wishes to acquire information on these obscure species, about which books will not assist him, he must not be content with a single lesson: he must have "line upon line, and precept upon precept." We have ourselves attempted some of them under the most skilful preceptors; and regret that the dark hints and general terms which they are used to employ do not enable us to profit much by their instruction. Undoubtedly a rich vocabulary and ample command of illustration will do something; but this only applies to the quantity. The point we are attempting to make is, that, after all, there is a limit to the communication of knowledge respecting the objects of Natural History, created not only by the imperfect nature of language, but by the evanescent impression which certain sensible characters leave upon the mind, thus furnishing materials for its own use, but which leave nothing behind that can be communicated to others.

Let us not be misunderstood. We are not blaming modern botanists for the course they have been taking. The results are only such as all minute analysis is necessarily subject to. It is an inconvenience produced by the imperfection of the instruments of thought, and until they are improved it is in vain to blame the naturalist for the consequences. It is however a question for his consideration, whether he cannot remedy part of the evil by some mark, or name, or arrangement of his type, which he might adopt for such species as are capable of being distinctly characterized by words, and such as are only known by habit and growth. To raise them all to the same rank is, in many genera, to involve them all in the same obscurity. The old species, so well known to our ancestors, are in danger of being lost, to be superseded by others which are obscure and undefinable. Students are frightened from the study by the difficulty they find in detecting any species; and the science is left in the hands of an eclectic number, who can only transmit it to their descendants by uncertain tradition; and if the tendency should be to restrict it to the few, instead of throwing it open to the many, we may be assured our mode of pursuing it is erroneous. This subject is important, and needs illustration to some extent; but we only hint at it here as introductory to our notice.

Sir James Edward Smith in his English Flora has from necessity adopted a great number of these recent obscure species, and which are not found in his *Flora Britannica;* not however without regretting the multiplication, yet finding it impossible to reject them, in consequence of the high credit on which they rested. The third volume does not contain so many as the two previous *; and, with some exceptions as to genera, the species are pretty much as the author's former works had left them. We will just notice the most prominent changes which have taken place. The *Nuphar minima* of Engl. Bot. is here very properly called *pumila*, a name which had been given by Hoffman previously to the publication of the figure in that work. The genus *Tilia*, which has been greatly confused, is revised thus: *T. Europæa* and *parvifolia* remain as before. *T. grandifolia* Ehrh. is adopted; and the *T. platyphyllos* of Ventenat, and the *T. ulmifolia semine hexagono* of Dillenius, are quoted under it, while the *T. corallina* of Rees's Cyclopædia, and Ray's Red-twigged Lime, are considered as a variety of it. *T. parvifolia* appears to us to be the only species found undoubtedly wild, the rest having been probably introduced as ornaments to our pleasure-grounds. The stations in Stoken Church Woods in Oxfordshire, it appears, cannot be relied on as wild, as many of the species now found there have the appearance of having been planted. Merrett's station for *T. grandifolia* in Surrey is in the same predicament: it is not found there in a natural wood. *Aconitum Napellus* is now first introduced as English; but it should, we apprehend, with the "Larkspur," have been marked with an asterisk, to indicate its doubtful claim to be indigenous. Under *Caltha palustris* is introduced a var. β, which DeCandolle has noticed, and which

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* The first and second volumes were noticed by us in vol. lxiii. pp. 219 and 284.

Miller had called *C. minor*. We have found it repeatedly on the mountains in Cumberland, and have seen it in herbaria mistaken for the *C. radicans*, which is a strongly marked and totally distinct species. Recent and authentic specimens of this last plant are, however, desiderata to the London botanists. The descendants of Dickson's original plants still survive; but whatever might be the authority of the finder, it is still desirable to have it confirmed.

Lamium maculatum wants confirmation even as an English plant: much more then does it need to be authenticated as found wild in woods in Scotland. *Stachys ambigua* appears to be confined to the North. It is but imperfectly known among Southern botanists; and that knowledge is derived from dried specimens, which in such difficult species are but unsatisfactory. The *Rhinanthus major* is entirely new. For this addition we are indebted to a very active and successful botanist, Mr. James Backhouse of York, who distinguishes it at first sight by its greater size, being two feet high, much branched and bushy; its much denser spikes; and its yellowish bracteas, each of which terminates in an elongated green point. The segments of the upper lip of the corolla are wedge-shaped and purple. Germen narrower and more tumid than in *R. Crista-galli*. Style prominent. Nectary heart-shaped, more spreading, and greenish. The seeds are thick at the edge, and not quite destitute of a membranous margin; but this is much narrower than in the former. Ehrhart and Richardson, in Dillenius, had previously distinguished the species. The *Linnæa borealis* seems to be more frequent in Scotland than had been imagined, though a single station for it has been discovered in England, by Miss Emma Trevelyan, at Hartburn in Northumberland, and recorded in the thirteenth volume of the Linnæan Transactions.

The most considerable alteration throughout the volume is to be found in the recasting the genera of the class *Tetradynamia*. In this the learned author has in part followed Mr. Brown, who was the first to point out the important characters afforded by the cotyledons; that is, whether they are flat, or folded, or spiral; whether *incumbent*, lying upon the embryo laterally, or, *accumbent*, their edges on one side meeting the embryo longitudinally. This Linnæan class, which comprehends one of the most natural orders throughout the vegetable kingdom, furnishes in consequence very obscure characters for subdivision. Linnæus was driven to rely upon the nectariferous glands for generic characters, and which, after all, did not enable the technical botanist to determine his plant; nor did it associate such species as were most nearly allied in habit. The characters employed by Mr. Brown are said to be easy of detection as soon as the skin of the seed is removed, there being no separate albumen; and these afford the most natural, and indeed absolute, primary characters of these plants. "They serve," says our author, "to divide the whole into great natural sections, liable, as far as I can find, to no exception; the genera under each section being easily characterized, and proving much more natural, in habit and fructification, than those found by Linnæus."

Whatever objection may, at first sight, appear against the use of these characters in the cotyledons, as furnishing little artificial assistance to the tyro, they are invaluable in the absence of more obvious marks, and confirm the empirical knowledge of habit and look, which we pointed out at the commencement of this paper as so much needed when we can no longer detect characters which can be described by botanical terms.

Matthiola incana is admitted here; but surely it is an escape from the gardens. At Hastings even double flowers may be observed. The *Malva pusilla* of Engl. Bot. is here reduced to a variety of *rotundifolia*. The *Orobus tenuifolius* of Roth, which Mr. D. Don had found in Scotland, Mr. Peete in Kent, and to these may be added, by ourselves in Glamorganshire, is regarded (and we think rightly) only as a variety of *tuberosus*. *Vicia angustifolia* of Sibthorp and others is introduced, and is no doubt a well-marked species. *Lotus decumbens*, an addition of Mr. T. F. Forster's in his *Flora Tonbridgensis*, is also new; while *L. diffusus* turns out to be *angustifolius* of Linnæus. *Medicago maculata*, *muricata*, and *minima*, first noticed by our author in the Cyclopædia, were before included in *M. polymorpha*.

In *Syngenesia* all the old *Hedypnoides* are placed under the genus *Apargia*. The Linnæan and Jussieuian genus *Cnicus* embraces many of our *Cardui*; but the author thinks the separation of these two genera justifiable only on the ground of convenience, and that they are not naturally separate. *Cnicus Forsteri*, (another discovery of our late estimable friend T. F. Forster, Esq., whose inquisitive eye seldom suffered a good plant to escape him,) if not absolutely distinct, is a singular hybrid, perhaps between *palustris* and *pratensis*. *Santolina* is now *Diotis*, upon the authority of Desfontaines and De Candolle. Under *Doronicum Pardalianche* our author does not quote the figure in the new series of the *Flora Londinensis*; and in this we think he is right, the plant there represented being *plantagineum*, which is, with the other, an occasional escape from gardens, as we have evidence from the very deserving and industrious botanist Mr. Baxter of the Oxford botanic garden, who received it a year or two ago from Brightwell in Berkshire, where it was found naturalized.

We lay down the volume, under a sense of the highest respect for its excellent author, and will venture again to express our earnest hope that he will not remit in his labour until he has completed the Flora of Great Britain, and thus supplied us with a text-book worthy of the advanced state of science.

Just published.

New Tables of Life Contingencies; containing the rate of mortality among the members of the Equitable Society, and the values of life annuities, reversions, &c. computed therefrom; together with extensive tables deduced from the Northampton rate of mortality, exhibiting the single and annual premiums for assurances on the joint existence, or last survivor, of two lives, or on one life against another, and the values of policies on single lives. To which are prefixed, a number of practical examples, illustrative of the application of the tables; and a new method of deducing the values of life annuities, &c. By Griffith Davies, actuary to the Guardian Assurance Company.

ANALYSIS OF PERIODICAL WORKS ON NATURAL HISTORY.

Zoological Journal. No. VII.

This number contains the following articles:—*Descriptions of thirteen Species of Formica, and three of Culex, found in the Environs of Nice, by* Dr. Leach.—*Descriptions of* Neotoma Floridana, *and* Sigmodon hispidum, *new mammiferous animals, of the order Glires, by* Messrs. Say and Ord: from the Journal of the Philadelphia Academy.—*Monograph of the Box Tortoises,* by Mr. Bell: a new genus, *Sternothærus,* is described in this monograph, which is thus characterized: "Sternum univalve: lobus anterior mobilis, lobi duo posteriores connexi, immobiles."—*On two Genera and several Species of Crinoidea, by* Mr. Say: from the Journal of the Philadelphia Academy.—*Additions to Mr. Say's paper on Crinoidea; Notice of a Fossil belonging to the Class Radiaria, found by* Dr. Bigsby *in Canada;* and *Descriptions of two new Species of the Genus Orbicula; by* Mr. G. B. Sowerby.—*On Leptophina, a group of Serpents comprising the Genus Dryinus of Merrem,* and *a newly formed Genus named Leptophis,* by Mr. Bell.—*Generic and Specific Characters of Ophidian, Chelonian, and Batrachian Reptilia, discovered by M. Spix in Brazil:* from the splendid works on the Brazilian Reptiles by Spix and Wagler.—*On the Genus Psaris of Cuvier, with an account of two new Species,* P. cristatus *and* P. niger, by Mr. Swainson.—*On the Isocardia Cor of the Irish Seas*, by the Rev. J. Bulwer, F.L.S.—*Description of some new British Shells*, by Dr. Turton: one of the shells described in this paper is generically new, and called *Galeomma;* being characterized as follows: "Testa bivalvis, æquivalvis, æquilateralis, transversa; margine antico ovato-hiante. Cardo edentulus. Ligamentum internum." The single species described by Dr. T., to which the conductors of the Journal have assigned the specific appellation *Turtoni*, was dredged up in the English Channel during a gale of wind; but Mr. Sowerby is stated to have two other species, one from the Mauritius and the other from Van Diemen's Land.—*Sketches in Ornithology*, by Mr. Vigors, comprising these sections,—*On the groups of the Vulturidæ*—*On a new genus of Falconidæ*—*On a new genus of Psittacidæ*, and *On the arrangement of the genera of Birds:* the last section consists of a list of the genera of Birds as they arrange themselves under their orders and families, in consonance with the views exhibited in the author's paper "On the Affinities of Birds," lately published in the Linnean Transactions.—*Analytical Notices of Books.*—The subjects described in the Number are illustrated by four plates, three of which are coloured.

XI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Jan. 12.—THE following papers were read: Observations on the heat of July 1825, together with some remarks on sensible cold, by W. Heberden, M.D. F.R.S.—Account of a series of observations to determine the difference of longitude between the national observatories of Greenwich and Paris, by J. F. W. Herschel, Esq. Sec. R.S.; communicated by the Board of Longitude.

Jan. 19.—On the Cambridge transit instrument, in a supplement to a former paper, by Robert Woodhouse, Esq. M.A. F.R.S. Plumian Professor of Astronomy in the University of Cambridge.—On the magnetic influence of the solar rays, by S. H. Christie, Esq. M.A. F.R.S.

Jan. 26.—On the barometer, by J. F. Daniell, Esq. F.R.S.

LINNÆAN SOCIETY.

Jan. 17.—Read a paper on some Cornish species of the genus *Labrus*, by Mr. Jonathan Couch, F.L.S. Among the species noticed were *Labrus Julis; Tinca* (Common Wrasse); *cornubiensis* (Goldsinny); *microstoma* (Corkwring); *trimaculatus; Comber; Perca inermis.*

GEOLOGICAL SOCIETY.

Nov. 18, 1825.—A notice was read respecting the appearance of fossil timber on the Norfolk coast, by Richard Taylor, Esq. of Norwich.

In consequence of an extraordinary high tide which visited the coast of Norfolk on the 5th of February last, large portions of the cliffs, sometimes exceeding 200 feet in height, were precipitated into the sea, and an opportunity was afforded of examining the site of a stratum containing a number of fossil trees exposed on the east and west sides of the town of Cromer. In this singular stratum, composed of laminæ of clay, sand, and vegetable matter, and about four feet in thickness, the trunks were found standing as thickly as is usual in woods, the stumps being firmly rooted in what appears to be the soil in which they grew. They are invariably broken off about a foot and a half from the base. The stem and branches lie scattered horizontally; and amongst them are thin layers of decomposed leaves, but no fruits or seed-vessels. The species of timber appear to be chiefly of the Pine tribe, with occasional specimens of elm and oak: they are flattened by the pressure of the overlying alluvial strata. Mr. Taylor has not observed any animal remains in the stratum, except a skull of one of the Deer tribe: but he supposes that the bones of elephants and other herbivorous animals found near this site may have been washed out of the same bed.

An extract of a letter from the Right Hon Earl Compton, F.G.S., to the President, was read, On the discovery of granite with green felspar found in excavations at Tivoli. In excavations made during the spring of 1825 at Tivoli, on the spot where the villa of Manlius Vopiscus stood, fragments of granite were discovered, the felspar of which is of a green colour, exactly resembling that which is called Amazonian stone. "As this rock was never before known to be among those employed by the ancients, it becomes a curious point," observes the author, "to ascertain whence they derived it, since the modern localities of the Amazonian stone are confined to Siberia and the continent of America." As Egyptian hieroglyphics appear on the original surface of some of these fragments, Lord Compton supposes the green granite to have been found, though a very rare substance, in Egypt.

A paper was also read entitled Notice of traces of a submarine forest at Charmouth, Dorset, by H. T. De la Beche, Esq. F.R.S., G.S., &c.—A circumstance, seeming to indicate the existence of the remains of a submarine forest near the mouth of the Char, was lately pointed out to Mr. De la Beche by Miss Mary Anning. Upon a flat of some extent, stretching into the sea in front of the beach, only visible at low water, and composed of lias, patches of a blue clay show themselves, imbedding pieces of blackened wood lying horizontally, similar in appearance to those usually met with in submarine forests: some of them are large, but the greater number must have been derived from small trees. Mixed with these are a few hazel-nuts, and abundant remains of plants, chiefly such as are found in marshy grounds. Angular and blackened pieces of chert and flint, precisely resembling those which occur in the diluvium on either side of the Char, form the substratum of this clay, which has been worn away in most places by the rolling of the large pebbles thrown up by the action of the sea upon the beach.

Dec. 2.—A paper entitled Remarks on the geology of Jamaica, by H. T. De la Beche, Esq. F.G.S., was read in part, &c.

A paper was also read entitled An account of an undescribed fossil animal from the Yorkshire Coal-field, by John Atkinson, F.L.S., and Edward Sanderson George, F.L.S.

Dec. 16.—A paper was read, On the chalk and sands beneath it (usually termed Green-sand), in the vicinity of Lyme Regis, by H. T. De la Beche, Esq. F.G.S. &c.

Mr. De la Beche observes, that we ought not to suppose that the sands, marles, and clays which are immediately subjacent to the chalk in the East of England, can be traced into other and distant countries, where however these sands, &c., as a mass, may be easily recognised. That this cannot be done even at comparatively short distances it is the object of this communication to prove, by examples derived from the cliffs at Lyme Regis in Dorsetshire, and Beer in Devonshire; detailed sections of which are given, and the succession of the strata and the organic remains which they contain fully described. The author first treats of the chalk, and the sands and sandstone usually called green-sand, as they occur between Lyme Regis and Axmouth, and then notices the same formations as they are exhibited in the vicinity of Beer.

From this examination it appears, that though there is a great correspondence in the organic remains, considerable changes take place in the mineral composition and characters of the beds both of chalk and underlying sands, in short distances. Mr. De la Beche considers it probable that the Beerstone is the equivalent of the Malm-rock of Western Sussex.

A paper was also read, entitled, A geological sketch of part of the West of Sussex, and the N.E. of Hants, &c., by R. J. Murchison, Esq. F.G.S. &c.

In this memoir Mr. Murchison describes the geological relations, distribution, and characteristic fossils of the strata of that part of the west of Sussex which is bounded on the south by the chalk escarpment of the South Downs, and that part of Hampshire which is included by the Alton chalk hills. These strata, commencing below the chalk, in a descending series, are, 1. Malm-rock, or upper green-sand.—2. Gault.—3. Ferruginous green-sand.—4. Weald clay. The Weald clay in the valley of Harting Combe may be regarded as the central nucleus of this district; mantling round which, and extending up to either chalk range, the other formations are developed in regular succession: the breadth and boundaries of each are laid down by the author on a coloured portion of the Ordnance Map, to which a section is annexed.

The Malm-rock of Western Sussex is identical with the stone of Merstham: it is characterized by constituting terraces which afford a rich soil favourable to wheat. It sometimes furnishes a building-stone, contains occasionally a calcareous blue chert, and abounds in organic remains.

The Gault of this district has been cut through to the depth of 120 feet, at Alice Holt, and iridescent Ammonites and other fossils are found in it. This clay is marked by fertile water-meadows; and the timber, presenting a green belt, clearly distinguishes it from the rich wheat land of the malm-rock above, and the arid expanse of the ferruginous green-sand below it.

Of this latter formation the upper beds consist of pure white sand, and in some places compact ironstone and ironstone in large cellular tubes are found in it. In the middle beds occurs a calcareo-siliceous grit, called Bargate-stone; in the lower, a siliceous yellow building-stone containing casts of Ammonites, Terebratulæ, &c.—The Weald clay includes in its middle beds the compact Petworth marble; and in lower beds of clay in which tabular calcareous grit occurs, Mr. Murchison has discovered, together with scattered shells of the *Vivipara Fluviorum*, the bones of a large unknown vertebrated animal, specimens and drawings of which accompany this memoir.

Jan. 6, 1826.—The reading of Mr. De la Beche's paper on the geology of Jamaica was continued.

MEDICO-BOTANICAL SOCIETY OF LONDON.

On Monday the 16th Jan. this Society held its anniversary meeting, when the following Officers and Council were elected for the present year:—*President*, Sir James M'Gregor, M.D.

M.D. F.R.S.—*Vice-Presidents*, William Thomas Brande, Esq.; Sir Astley Cooper, Bart. F.R.S.; Sir Alex. Crichton, F.R.S.; Sir William Franklin, F.R.S.; Edward Thomas Munro, M.D.; John Ayrton Paris, M.D. F.R.S.—*Treasurer*, Henry Drummond, Esq. F.S.A.—*Secretary*, Richard Morris, Esq. F.L.S.—*Director*, John Frost, Esq. F.S.A.—*Auditor of Accounts*, William Newman, Esq.—*Council*, The President, Vice-Presidents, and other officers; together with Thomas Gibbs, Esq. F.H.S.; Theodore Gordon, M.D. M.R.A.S.; Thomas Jones, Esq.; George H. Roe, M.D.; John Gordon Smith, M.D.; William Yarrell, Esq. F.L.S.

The gold medal of this Society was awarded to Matthew Curling Friend, Esq. Lieutenant in the Royal Navy and F.R.S., for his communication respecting certain articles of Materia Medica used in Africa: and the silver medal to James Hunter, Esq. F.H.S.

ROYAL ACADEMY OF SCIENCES OF PARIS.

Aug. 8.—M. de Monferrand, professor at the Royal College of Versailles, wrote to the Academy with the design of showing that the properties of curves of the second degree, respecting which M. Hachette had communicated a paper, were already known.—M. Dupetit-Thouars read a notice on the dilatation which slips of the White Poplar sometimes undergo.—A memoir by MM. Quoy and Gaimard was read, entitled Observations on certain Crustacea, considered with regard to their habits and geographical distribution; succeeded by the description of some new species discovered during M. Freycinet's circumnavigation of the globe.—Dr. Lassis read a notice on the epizooty of 1815, and on that of the present year; and also the continuation of another notice on the causes of epidemics.

Aug. 16.—M. Dupin read a notice on a new precept in geometry and mechanics, applied to the arts.—MM. Vauquelin and Thenard made a favourable report on the memoir of MM. Bussy and Lecanu, entitled On the action of heat on the fatty bodies; and on that by M. Dupuy, On the distillation of those substances.—M. de Lacépède presented the new statutes of the University of New York, together with some meteorological observations made at Albany in that state.—M. Moreau de Jonnès read a note on the official inquiries respecting the contagion of the yellow fever and the plague.—M. Marion read a memoir on cauterization in smallpox and other eruptive disorders.

Aug. 22.—M. Bressy, a physician at Arpajon, transmitted to the Academy two pairs of spectacles which he calls *rostral* spectacles.—Drs. Laserre and Costa communicated some critical remarks on M. Moreau de Jonnès's note read as above.—M. Arago communicated extracts from two letters relative to the late appearance of two comets.—M. Mathieu, in the name of a committee, read a favourable report on a memoir of perspective geometry, or a new method of describing bodies geometrically, by M. Cousinéry.—M. Lonchamp read a memoir on the effects of a high temperature applied to the evaporation of liquids.—M. Julia-Fontanelle read a memoir on the native hydrate of sulphur discovered in the department of the Aude.—M. Arago, in the name of a committee, gave a favourable report on the voyage of discovery made from 1822 to 1825 under the command of Lieut. Duperrey.

Aug. 29.—M. Berard, of Briançon, communicated a new memoir on the theorem of Fermat.—Dr. Lassis addressed a letter to the Academy on the contagion of Typhus.—M. Magendie presented a memoir on Hydrophobia, by Dr. Marochetti.—MM. Cuvier and Duméril made a favourable report on M. Barry's memoir relative to the action of the atmosphere on respiration.—M. Civiale read a memoir on *lithontripty*, or the new method of breaking the stone in the bladder.

XII. *Intelligence and Miscellaneous Articles.*

NATURAL FORMATION OF VARIOUS METALLIC OXIDES AND SALTS.

THE following is an abstract of a paper on this interesting subject, read before the Royal Society on the 17th of November last: several other cases of the same nature will be found in our last volume, pp. 153 and 395.

On the Changes that have taken place in some ancient Alloys of Copper; in a letter from John Davy, M.D. F.R.S., to Sir Humphry Davy, Bart. Pres. R.S.—In this letter Dr. Davy, who is pursuing a train of scientific researches in the Mediterranean, describes the effects which time and the elements have produced on various Grecian antiquities. The first he examined was a helmet of the antique form found in a shallow part of the sea between the citadel of Corfu and the village of Castrades, which was partly covered with shells and with an incrustation of carbonate of lime. Its entire surface, as well where invested with these bodies as where they were absent, presented a mottled appearance of green, white, and red. The green portion consisted of the submuriate and the carbonate of copper, the white chiefly of oxide of tin, and the red of protoxide of copper in octahedral crystals, mingled with octahedrons of pure metallic copper. Beneath these substances the metal was quite bright, and it was found by analysis to consist of copper and 18·5 per cent of tin. A nail of a similar alloy from a tomb at Ithaca, and a mirror from a tomb at Samos, in Cephalonia, presented the same appearances, but in less distinct crystallization: the mirror was composed of copper alloyed with about six per cent of tin, and minute portions of arsenic and zinc. A variety of ancient coins, from the cabinet of a celebrated collector at Santa Maura, presented similar appearances, and afforded corresponding results; the white incrustations being oxide of tin, the green consisting of carbonate and submuriate of copper, and the red of the protoxide of the same metal; some having a dingy appearance arising from the presence of black oxide of copper mingled with portions of the protoxide. Dr. Davy was unable to detect any relation between the composition of the respective coins and their state of preservation, the variation in this respect which they presented appearing to arise rather from the circumstances under which they had been exposed to the mineralizing agents. In conclusion, Dr. Davy observed, that as the substance from which these crystalline compounds had been produced could not be imagined to have been in solution, their formation must be referred to an intimate motion of its particles, effected by the conjoint agency of chemical affinities, electro-chemical attraction, and the attraction of aggregation. He suggested the application of this inference to explain various phænomena in mineralogy and geology.—*Annals of Philosophy.*

MAGNETIC ROTATION.

M. Arago's beautiful experiment is now well known, and, as it deserves, attracts attention every where. The following are some results obtained by MM. Prevost and Colladon, which, as they vary slightly in certain points from those as yet published in this country, will be interesting to such as pursue this branch of science.

A disc formed of a thick copper wire rolled in a spiral, produced much less effect than a perfect disc of the metal of the same weight and size.

A disc of glass covered with lead, or a single leaf of tin glued on to wood, sensibly deviated the needle. Wood alone, or sulphur, or a disc of peroxide of iron, had no appretiable effect.

A disc of hammered copper deviated the needle more strongly than the same disc annealed.

A screen of copper, or copper and zinc interposed, diminished the effect without destroying it. The diminution was greater as the screen was thicker, or placed nearer to the needle. A screen of glass had no influence. If the interposed metallic screen were pierced by an aperture equal in diameter to the length of the needle, its effect was very nearly the same.

A vertical magnet suspended in the centre of a cylinder of copper remained unmoved, whatever the direction or rapidity of rotation of the ring.

When two needles were fixed together in a similar direction, the effect increased; when they were placed with their opposite poles together, it ceased entirely.

A needle magnetized, so as to have similar poles at its two extremities, was the apparatus most sensible to the motion of the discs. It was one of this kind which the authors used in their delicate experiments.

The conclusion arrived at by MM. Prevost and Colladon is, that the effects are due to a transient magnetization of the discs, which, not being able to modify itself with a rapidity proportional to that by which the different points of the disc are displaced by rotation, are transported to a small angular distance from the needle before they are changed, and draw it after them. This is the same explanation in effect as that of MM. Herschel and Babbage.

Experiments made with care to determine the influence of the velocity and the distance of the discs, indicated that the angles of deviation, and not their sines, augmented proportionally with the velocity, at least, within certain limits, and that the sines of the angles of deviation increased in an inverse ratio of the power $2\frac{2}{10}$ of the distance. They were careful to employ, in this determination, discs having diameters very great in comparison to the length of the needle.—*Bib. Univ.* xxix. 316.

NECESSITY OF WATER IN THE PREPARATION OF LEAD-PLASTER.

Attempting to form lead-plaster, the *Emplastrum Plumbi* of the *Pharmacopœia*, without the use of water, steam being the source of heat, I was surprised to find after several hours, during which time the litharge and oil had been kept at a temperature of 220°, or thereabout, and constantly stirred, not the slightest appearance of combination; upon the addition of a small quantity of boiling water, the oil and oxide immediately saponified: water appeared, therefore, to be essential to the formation of the plaster. It also appeared probable the oxide might be in the state of hydrate. To ascertain if such were the case, I precipitated, by potash, the oxide from a quantity of acetate; the precipitate, when washed, was dried by a heat of 220° until it ceased to lose weight. 100 grains, heated to redness in a tube, gave off nearly 8 grains of water, and assumed the orange-colour of litharge: the recently precipitated oxide was no doubt, therefore, an hydrate; part of which, with somewhat less than two parts of olive oil, without any addition of water, at a temperature of 212°, formed, in half an hour, perfect plaster. Each of these experiments has been repeated with precisely the same results. I am induced to mention this fact, because all pharmaceutical writers limit the action of the water to that of keeping down the temperature. H.H.—*Journal of Science.*

LIST OF NEW PATENTS.

To John M'Curdy, of Cecil-street, Strand, esquire, for improvements in generating steam.—Dated 27th Dec. 1825.—6 months to enrol specification.

To James Ogston and James Thomas Bell, of Davies-street, Berkley-square, watchmakers, for improvements in the construction or manufacture of watches, communicated from abroad.—6th January, 1826.—2 months.

To Richard Evans, of Bread-street and Queen-street, Cheapside, for improvements in the apparatus for and process of distillation.—7th Jan.—6 months.

To Henry Houldsworth junior, of Manchester, for improvements in machinery for giving the taking-up or winding-on motion to spools or bobbins, &c. on which the roving or thread is wound in roving, spinning, and twisting machines.—16th Jan.—6 months.

To Benjamin Newmarch, of Cheltenham, esquire, for his improved method of exploding fire-arms.—16th Jan.—6 months.

To John Rothwell, of Manchester, tape-manufacturer, for his improved heald or harness for weaving purposes.—16th Jan.—2 months.

To Henry Anthony Koymans, of Warnford-court, Throgmorton-street, for improvements, communicated from abroad, in the construction and use of apparatus and works for inland navigation.—16th Jan —6 months.

To John Frederick Smith, of Dunston Hall, Chesterfield, Derbyshire, esquire, for an improvement in drawing, roving, spinning and doubling wool, cotton, &c.—19th Jan.—6 months.

To William Whitfield, of Birmingham, for improvements in making of handles for saucepans, kettles, &c.—19th Jan.—6 months.

To Benjamin Cook, of Birmingham, brass-founder, for improvements in making hinges.—19th Jan.—6 months.

To Abraham Robert Lorent, of Gottenburg, Sweden, merchant, at present residing in King-street, Cheapside, for a method of applying steam without pressure to pans, boilers, coppers, stills, pipes, and machinery, in order to produce, transmit, and regulate various temperatures of heat in the processes of boiling, distilling, evaporating, inspissating, drying, and warming, and also to produce power.—19th Jan.—6 months.

To Sir Robert Seppings, knight, a commissioner and surveyor of the navy, of Somerset House, for his improved construction of such masts and bowsprits as are generally known.—19th Jan.—2 months.

To Robert Stephenson, of Bridge Town, Stratford, Warwickshire, engineer, for axletrees to remedy the extra friction on curves to carriages used on rail-roads, train-ways, and other public roads.—23d Jan.—6 months.

SUMMARIES OF METEOROLOGICAL OBSERVATIONS FOR THE PAST YEAR;

[Continued from vol. lxv. p. 77.]

Results of a Meteorological Register kept at New Malton, in the N. R. of Yorkshire, in the Year 1825, *by* JAMES STOCKTON, *Esq. Corr. Mem. Met. Soc.*

Latitude 54° 8′ 3″. Longitude 0° 47′ 4″ W.

Height of the Cistern of the Mountain Barometer above the Level of the Sea 92 feet.

The Funnel of the Rain-gauge is 7, and the Thermometer 3 feet from the Ground.

1825.	Barometer.						Thermometer.				Winds.											Weather.			Rain.	
Months.	Max	Min.	Mean.	Range.	Spaces described in Inches.	No. of Changes.	Max.	Min.	Mean.	Range.	N.	NE.	E.	SE.	S.	SW.	W.	NW.	Var.	Brisk.	Boist.	Rain.	Snow.	Hail.	in Inches, &c.	Character of each Month.
Jan.	30·80	29·01	30·086	1·79	8·47	18	52°	25°	37°·161	27°	5	1	1	...	2	12	4	6	...	2	1	4	2	...	1·62	Cloudy, dry and temperate
Feb.	30·45	29·21	30·032	·24	5·82	14	51	22	36·935	29	2	...	...	...	8	11	3	4	...	...	2	2	2	...	·89	Ditto
March	30·55	29·09	30·072	·46	5·01	12	60	26	39·806	34	11	6	3	3	4	2	...	1	1	1	...	3	3	...	1·44	Cloudy, and wet at the begin.
April	30·50	29·35	29·943	·15	3·70	13	64	30	47·450	34	5	4	1	2	...	6	5	3	4	7	1	5	...	1	1·98	Dry to the 20th, then wet
May	30·30	29·43	29·906	·87	3·63	12	74	32	52·113	42	7	7	5	2	3	1	1	...	5	1	...	12	...	1	3·25	Wet and changeable
June	30·40	29·14	29·903	·26	4·15	11	80	41	57·700	39	4	3	...	...	6	8	4	...	5	3	2	10	...	1	2·44	Warm and chiefly dry
July	30·29	29·74	29·103	·55	2·26	11	88	42	63·048	46	14	6	...	1	2	3	4	1	...	2	...	1	...	...	·42	Clear, hot, and dry
August	30·26	29·12	29·838	·12	4·30	10	84	42	61·000	42	6	2	3	1	3	6	7	1	2	4	2	11	...	...	4·34	Hot and wet
Sept.	30·33	29·40	29·790	·93	3·87	14	75	41	59·333	34	4	2	1	6	5	5	2	2	3	5	2	7	...	...	1·39	Fine and warm
Oct.	30·30	28·91	29·830	·39	6·27	10	66	29	46·500	37	1	...	...	3	4	16	4	3	...	6	3	11	1	...	3·12	Wet, and changeable
Nov.	30·15	28·55	29·577	·60	9·02	18	54	25	38·500	29	3	1	...	...	7	8	6	5	...	7	6	15	2	1	3·20	Wet, stormy, and changeable
Dec.	29·84	28·80	29·457	1·04	5·54	13	51	18	38·516	33	5	4	...	1	5	11	...	4	1	3	3	11	2	...	3·28	Wet, damp, and cloudy
Annual Means, &c.	30·80	28·55	29·878	2·25	64·04	156	88	18	48·171	70	67	36	14	19	49	89	40	30	21	41	22	92	12	4	27·37	

ANNUAL RESULTS.

Barometer.

	Inches.
Highest observation, Jan. 9th. Wind N. ...	30·800
Lowest observation, Nov. 3d. Wind N.W. ...	28·550
Range of the mercury	2·250
Mean annual barometrical pressure	29·878
Greatest range of the mercury in January	1·790
Least range of the mercury in July	·550
Mean monthly range of the mercury	1·200
Spaces described by the different oscillations ...	64·040
Total number of changes in the year	156·000

SIX'S *Thermometer.*

Greatest observation, July 18th. Wind N. ...	88°000
Least observation, December 31st. Wind N. ...	18 000
Range of the mercury in the thermometer ...	70 000
Mean annual temperature	48 171
Greatest range in July	46 000
Least range in February and November	29 000
Mean monthly range	35 500

Winds.

	Days.		Days.
North	67	West	40
North-East	36	North-West	30
East	14	Variable	21
South-East	19	Brisk	42
South	49	Boisterous	22
South-West	89		

Rain, &c.

	Inches, &c.
Greatest quantity in December	3·280
Least quantity in July	0·420
Total amount for the year	27·370
Days of rain	92·000
Days of hail	4·000
Days of snow	12·000

J. S.

New Malton, January 6, 1826.

Summary for the Year 1825, *of the State of the Barometer, Thermometer, &c. in Kendal. By* S. MARSHALL, *Esq.*

1825.	Barometer.			Thermometer.			Quantity of Rain in Inches.	No. of rainy Days.	Prevalent Winds.	Quantity of Rain in Inches in 1824.
	Max.	Min.	Mean.	Max.	Min.	Mean.				
1st Month	30·38	28·52	29·61	49°5	20°5	35°72	5·932	16	NW.	3·908
2d Month	30·14	28·88	29·57	48	23	37·66	5·524	9	SW.	2·906
3d Month	30·38	29·03	29·87	58·5	25	39·42	2·962	11	SW.	6·301
4th Month	30·25	29·09	29·76	65	27	45·51	2·210	12	SW.& NW.	2·377
5th Month	30·10	29·30	29·71	70·5	32	51·21	4·096	13	SW.	·681
6th Month	30·14	29·08	29·70	80	38	55·50	6·333	14	W.	2·034
7th Month	30·09	29·29	29·86	85	38	60·39	·701	4	W.	1·721
8th Month	30·08	29·05	29·65	80	42	59·75	4·558	13	SW.	2.977
9th Month	30·10	29·23	29·59	72	37	57·25	6·624	18	SW.	5·619
10th Month	30·07	28·86	29·66	66	29	49·79	6·993	24	SW.	7·598
11th Month	29·98	28·45	29·41	52·5	20	38·82	10·028	19	SW.	13·433
12th Month	29·69	28·76	29·28	51	21	38·95	4·012	16	SW.	13·207
Average.			29·54			47·49	59·973	169		62·762

Remarks.—The mean height of the barometer during the present year was greatest in the month of March (though in this part of the country that is usually the case in January), and least in December. The mean temperature exceeds that of 1824 merely by half a degree. During the summer months the heat greatly exceeded that of last year; but towards the beginning and end of this, the weather has been more severe, which has tended nearly to equalize the annual means of 1824 and 1825.

The quantity of rain has fallen short of the last three years by nearly three inches, though it is still above the average for Kendal. In July, which is generally a wet month, there fell only ·701 inch, and in November and December 14·030 inches. In these two months of 1824, 26·640 inches were taken, which is an unusually large quantity. The number of wet days in the present has fallen short of those in the last year, being 169; but in 1824 there were 187.

For eight months the prevalent wind was SW., which may be concluded, from preceding observations, to be decidedly the prevalent wind of Kendal. S. M.

METEOROLOGICAL TABLE.

Extracted from the Register kept at Kinfauns Castle, N. Britain. Lat. 56° 23′ 30″.—Above the level of the Sea 140 feet.

1825.	Morning, 10 o'clock. *Mean height of*		Evening, 10 o'clock. *Mean height of*		Mean Temp. by Six's	Depth of Rain.	N° of Days.	
	Barom.	Ther.	Barom.	Ther.	Ther.	Inch. 100.	Rain or Snow.	Fair.
January ..	29·961	39·387	29·936	39·935	40·355	1·45	9	22
February..	29·912	39·928	29·893	39·250	40·071	0·95	9	19
March	29·992	41·742	29·978	40·161	41·709	1·20	10	21
April	29·854	47·300	29·835	43·600	46·700	2·40	9	21
May	29·873	51·322	29·897	47·097	50·096	2·60	13	18
June .	29·785	57·566	29·764	53·000	56·500	2·50	9	21
July......	30·010	63·097	30·020	58·129	62·032	0·30	5	26
August ...	29·733	61·322	29·725	57·485	60·838	2·00	9	22
September.	29·715	58·600	29·701	54·866	57·600	2·35	16	14
October...	29·678	51·322	29·671	48·903	51·161	2·15	14	17
November.	29·451	41·400	29·417	39·833	41·066	2·80	9	21
December.	29·412	40·677	29·437	40·484	40·451	3·20	17	14
Average of the year.	29·781	49·742	29·773	46·895	49·048	23·90	129	236

ANNUAL RESULTS.

MORNING.

Barometer. *Observations.*		*Wind.*		*Thermometer.*	*Wind.*	
Highest,	9th Jan.	SW.	30·80	16th June,	SW. . .	71°
Lowest,	18th Jan	E.	28 66	31st December.	W. . .	25°

EVENING.

Highest,	9th Jan.	SW.	30·75	30th July,	SE. . .	66°
Lowest,	5th Nov.	SE.	28 64	31st December,	W. . .	26°

Weather.	*Days.*
Fair	236
Rain or Snow . .	129
	365

Wind.	*Times.*
N. and NE.	9
E. and SE.	119
S. and SW.	95
W. and NW.	142
	365

Extreme Cold and Heat, by Six's Thermometer.

Coldest, 31st December . .	Wind	W. . . .	21°
Hottest, 18th July	Wind	W. . . .	80°
Mean Temperature for 1825			49° 048′

RESULT OF TWO RAIN GAUGES.	In. 100
1. Centre of Kinfauns Garden, about 20 feet above the level of the Sea	23·90
2. Square Tower, Kinfauns Castle, about 140 feet	23·45

METEORO-

METEOROLOGICAL OBSERVATIONS *in London, and of Dr. BURNEY at Gosport, omitted in the Number for November.*

	London	Gosport, at half-past Eight o'Clock, A.M.							Clouds.						
Days of Month, 1825.	Barom. ½ past 8. Inches.	Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.
○ Oct. 26	30·02	30·10	40	55·00	70	NW.	...	0·145	...	...	1	...	...	...	1
27	29·90	30·00	49	......	72	N.	...	......	...	...	1	...	...	1	...
28	29 94	30·00	53	......	76	NW.	0·15	......	...	...	1	...	...	1	1
29	29·92	30·06	56	......	80	W.	...	·105	...	1	1	...	1	1	1
30	29·90	30·05	52	......	72	W.	...	·020	1	1	1	...	...	1	1
31	29·80	30·02	48	55·00	76	W.	·15	......	1	1	1	...	...	1	...
Nov. 1	29·87	30·00	55	......	81	W.	...	......	1	1	1	...	...	1	1
2	29 72	29·85	48	......	82	W.	...	·100	1	1	1	...	1	1	1
☾ 3	28·93	29·11	54	......	72	SW.	·30	·025	...	1	1	...	1	1	1
4	29·45	29·62	41	......	72	NW.	...	......	1	...	1	...	1	...	...
5	29·90	29·95	36	......	73	NW.	...	·165	1	1	1	...	1	1	1
6	29·15	29·29	55	......	87	SW.	·20	·110	1	1	1	...	...	1	1
7	29·20	29 26	41	......	71	NW.	...	......	...	...	1	...	...	...	..
8	29·32	29·36	40	54·60	67	E.	...	·820	...	...	1	...	...	1	1
9	29·02	29·19	44	......	69	W.	·10	1 170	1	1	1	...	1	1	1
10	28·76	28 60	42	......	100	NE.	...	·510	...	...	1	...	..	1	1
11	29·32	29·34	38	......	85	N.	...	......	...	...	1	...	...	1	1
12	29·75	29·77	37	......	71	N.	·12	......	...	...	1	...	...	...	...
13	29·85	29·88	31	......	82	N.	...	......	...	...	1	1	...	...	...
14	29·80	29·88	43	......	74	NW.	...	·010	...	...	1	...	1	1	1
15	30·00	30·05	38	54·10	75	N.	·17	......	1	...	...	...	1	...	...
16	30·11	30·17	37	......	81	NW.	...	·010	...	...	1	...	...	1	1
17	30 04	30·08	40	......	86	E.	...	·020	...	...	...	...	1	1	1
18	29·90	29 97	44	......	90	S.	·09	·030	...	...	1	...	...	...	1
19	29·80	29·88	45	......	92	W.	...	·010	...	1	1	...	1	1	1
20	30 10	30·27	38	53 70	88	W.	...	......	1	1	1	...	...	1	1
21	29·70	29·90	51	......	91	NW.	·13	·080	1	...	1	...	...	...	1
22	29·82	29·95	40	......	87	NW.	...	......	...	...	1	...	...	...	1
23	30·12	30·30	40	......	86	W.	...	·085	...	...	1	...	...	1	1
24	30·11	30·20	49	......	90	SW.	...	·160	...	...	...	...	...	1	1
○ 25	30·16	30·22	42	53·15	88	NW.	0·15	......	1	...	...	...	1	...	...
Aver. :	29·21	29·817	44·10	54·26	80·2		1·56	3·575	12	11	27	1	11	21	23

OBSERVATIONS *in Gosport, London, and Boston; continued from the last Number to the end of* 1825.

	Gosport, at half-past Eight o'Clock, A.M.							Clouds.						
Days of Month, 1825.	Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground.	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.
Dec. [illegible]	[illegible]9·95	40	51·60	90	W.	......	0·030	1	1	1	...	...	1	1
27	29·[illegible]7	30	......	90	NW.	......	......	...	...	1	...	...	...	...
28	29·[illegible]	26	......	86	NW.	0·12	......	...	...	1	...	1	1	...
29	29·5[illegible]	32	......	90	NE.	......	......	...	...	1	...	...	1	...
30	29·6[illegible]	29	......	87	NW.	......	......	1	...	1	1	...	1	1
31	29·76	26	51·35	84	NW.	·14	220	...	1	1	1	1	1	1
Aver.	29·740	30·50	51·47	87 8		0 26	0·250	2	2	6	2	2	5	3

Days of Month, 1825.	Height of Barometer, in Inches, &c.		Thermometer.				Rain.		Weather.		
	Lond. 1 P.M.	Bost. 8½ A.M.	London. 8 A.M.	London. Noon.	London. 11 P.M.	Boston. 8½ A.M.	London.	Boston.	London.	Boston.	Wind.
Dec. 26	29·82	29·55	38	42	40	38 5	...	...	Fair	Snow *p.m.*	SW.
27	29·89	29·42	30	32	28	33	...	...	Snow		W.
28	29·70	29·42	32	38	34	33	...	...	Cloudy		W.
29	29·66	29·40	33	34	34	31·5	...	...	Foggy		W.
30	29·75	29·45	30	33	29	31·5	...	...	Fair	Snow *p.m.*	W.
31	29·79	29 50	28	32	33	32	.	·17	Fair		SW.

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XIII. *On the Theory of the Figure of the Planets contained in the Third Book of the* Mécanique Céleste. *By* J. Ivory, *Esq. M.A. F.R.S.*

[Concluded from p. 37.]

II. THERE can be no other apology for the observations which I have made on the analysis of Laplace, except that they are true and rigorously proved. And as this is the best apology that can be made, so I am not aware that any other is necessary. To avoid as much as possible all objection and cavil, I have employed in the proof the author's own mode of investigation. Speculations of this kind are at present entirely out of fashion, or rather they are discouraged and undervalued as much as possible. They seem even to be excluded from what is popularly called the inductive philosophy, forgetting that they form a part of the noblest and most successful induction that, we may venture to predict, will ever do honour to the human intellect. What has occupied the attention of Maclaurin and Simpson, of D'Alembert, Lagrange and Laplace, is now utterly condemned as useless, with a degree of levity that will not easily be believed. But the exclusive spirit which reigns so powerfully at present, whether it proceeds from particular interests or from narrow views of science, will at length spend its force; and the discussion I have undertaken may then contribute to place an important branch of the philosophy of Newton on a solid foundation. It follows from what has been shown, that the method of Laplace, when freed from series without convergency, and reduced to what is strictly demonstrative, is confined to a class of spheroids first proposed by D'Alembert. We cannot, therefore, allow that the method is perfectly general, unless it were proved that the class of spheroids mentioned comprehends every case in which the conditions of equilibrium can possibly be fulfilled. But it is greatly to be wished that so important a part of the system of the world as the theory of the figure of the planets, were deduced from sure principles, by a process of reasoning not depending upon any dubious or intricate point of analysis.

In treating of the figure of the planets there are three different cases that principally engage attention. We may consider the equilibrium of a fluid mass that is homogeneous; or of one composed of strata varying in density according to any law; or we may suppose a solid nucleus wholly or partially covered with a fluid. Although the principles on which I proceed are equally applicable in every case, yet for the sake of brevity and simplicity I here confine myself to the first case only; namely, the equilibrium of a homogeneous mass of fluid. Such is the intimate connexion which binds together the different parts of the same theory, that if we can fairly overcome the difficulties which obstruct our progress in one case, every other case will readily be brought within our power.

Now the principles by which Laplace has determined the equilibrium of a homogeneous fluid mass are these two: first, the direction of gravity must be every where perpendicular to the outer surface; secondly, the radius r of the spheroid must come under this formula, viz. $r = a(1 + \alpha y)$, y being a function of the angles which determine the position of the radius, and α a small coefficient of which the square and higher powers are to be neglected. In the theory of Laplace, the equilibrium of a fluid of uniform density is a necessary consequence of the two conditions mentioned. Of these the first is entirely mathematical; the only purpose it can serve is to allow the rejecting of certain quantities which would otherwise embarrass calculation; but it can in no respect contribute to make out the proof of the equilibrium, which must be deduced from hydrostatical principles alone. Whether there be an equilibrium or not must depend entirely on the first condition. If that is sufficient, Laplace's solution will be exact; otherwise we must conclude that it is defective, and we can consider it only as a method of calculation which accidentally leads to a result that we know to be true from other considerations.

We have now then to inquire what are the conditions necessary to the equilibrium of a homogeneous fluid. The whole received doctrine on this head is contained in the single proposition following. Conceive a homogeneous fluid contained within a continuous surface, and let x, y, z denote the rectangular co-ordinates of a point in the surface drawn to three planes intersecting in the centre of gravity of the mass; then, ϕ denoting a function of x, y, z, if the equation of the outer surface be

$$\phi = C;$$

the fluid will be *in equilibrio*, if every molecule, whether situated in the surface or any where in the interior of the fluid, is urged by the forces $\frac{d\phi}{dx}$, $\frac{d\phi}{dy}$, $\frac{d\phi}{dz}$ in the direction of the co-ordinates and tending to shorten them, it being always understood that the co-ordinates of the molecule are to be substituted in the expressions of the forces.

In order to demonstrate this proposition take the differential of the equation of the surface; then

$$\frac{d\phi}{dx}dx + \frac{d\phi}{dy}dy + \frac{d\phi}{dz}dz = 0:$$

now $\frac{d\phi}{dx}$, $\frac{d\phi}{dy}$, $\frac{d\phi}{dz}$ are the forces which act upon a molecule in the surface at the point of which x, y, z are the co-ordinates; and if we put

$$p = \sqrt{\left(\frac{d\phi}{dx}\right)^2 + \left(\frac{d\phi}{dy}\right)^2 + \left(\frac{d\phi}{dz}\right)^2},$$

then p, which is the resultant of the partial forces, will represent the gravity at the exterior surface; and it is easy to deduce from the differential equation that the direction of p will be perpendicular to that surface. Suppose that the constant quantity C in the equation of the fluid's surface decreases by a small variation, then

$$\phi = C - \delta C,$$

which will be the equation of a surface in the interior of the mass indefinitely near the outer surface: and since the forces which urge every molecule of the fluid are expressed by the same functions of the co-ordinates of the molecule, it follows that the resultant of the forces acting at any point of this new surface will be perpendicular to it, for the same reason that the like resultant is perpendicular to the outer surface. And as we may conceive that C decreases by indefinitely small gradations till it is entirely exhausted, it is evident that the whole fluid mass may be supposed to be divided into thin strata separated from one another by surfaces perpendicular to the forces which urge the molecules contained in them. Clairaut has called such surfaces *Couches de niveau*, or level surfaces, from the property which they possess in common of being perpendicular to the direction of gravity. Again, let k denote the perpendicular distance between the outer surface of the fluid and the level surface immediately below it; and let x, y, z be the co-ordinates of the extremity of k in the first surface, and $x - \delta x$, $y - \delta y$, $z - \delta z$ the co-ordinates of the other extremity in the other surface; then if we substitute the respective co-ordinates in the equations

$$\phi = C$$
$$\phi = C - \delta C,$$

and take the difference, we shall get

$$\frac{d\varphi}{dx}\,\delta x + \frac{d\varphi}{dy}\,\delta y + \frac{d\varphi}{dz}\,\delta z = \delta C.$$

As before, put

$$p = \sqrt{\left(\frac{d\varphi}{dx}\right)^2 + \left(\frac{d\varphi}{dy}\right)^2 + \left(\frac{d\varphi}{dz}\right)^2},$$

and it will be easy to prove that the cosines of the angles which the lines δx, δy, δz make with k are respectively equal

to $$\frac{1}{p} \times \frac{d\varphi}{dx},\ \frac{1}{p} \times \frac{d\varphi}{dy},\ \frac{1}{p} \times \frac{d\varphi}{dz};$$

hence

$$\delta x = \frac{k}{p} \times \frac{d\varphi}{dx},\ \delta y = \frac{k}{p} \times \frac{d\varphi}{dy},\ \delta z = \frac{k}{p} \times \frac{d\varphi}{dz}:$$

wherefore, by substituting these values in the foregoing expression, we shall obtain

$$\frac{k}{p} \times \left\{\left(\frac{d\varphi}{dx}\right)^2 + \left(\frac{d\varphi}{dy}\right)^2 + \left(\frac{d\varphi}{dz}\right)^2\right\} = k \times \frac{p^2}{p} = kp = \delta C.$$

Suppose, further, that the level surface is divided into equal elementary parts, one of which is ds; then $k \times ds$ will represent the small mass of fluid contained between the two surfaces upon the base ds; and, p being the resultant of the forces that urge every molecule of the small mass, $k \times p \times ds = ds \times \delta C$ will represent its pressure upon the level surface: wherefore, since δC and ds are the same at every point of the surface, it follows that the pressure will be equal upon all equal spaces of the level surface. In the very same manner it may be shown that the second stratum presses equally upon the level surface below it, and the same thing is manifestly true of all the successive strata in order. But as the number of strata increases, the fluid within them continually decreases in quantity; ultimately therefore it will be reduced to a particle which exerts no force, and which, being pressed equally on all sides, cannot fail to be *in equilibrio.* Remounting now from this central drop to the exterior surface, it is evident that the whole mass of fluid, and every part of it bounded by a level surface, will be separately *in equilibrio.*

In the foregoing demonstration we have followed the ideas of Clairaut in his excellent work on the figure of the earth*. It appears from the reasoning that all the conditions of equilibrium are contained in the equation of the external surface, viz.

$$\varphi = C,$$

nothing more being necessary than that φ be a function of three independent co-ordinates. Instead of the equation itself we may substitute its fluxion, viz.

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* *Figure de la Terre,* prem. partie, § xxi.

$$\frac{d\varphi}{dx}dx + \frac{d\varphi}{dy}dy + \frac{d\varphi}{dz}dz = 0;$$

which again amounts to affirming that the resultant of the forces $\frac{d\varphi}{dx}$, $\frac{d\varphi}{dy}$, $\frac{d\varphi}{dz}$ urging a molecule in the external surface must be perpendicular to that surface.

From what has now been shown, it appears that Laplace in his investigation has strictly adhered to the received theory of fluids. But the algebraic calculus, in its generalizations, is apt to overlook distinctions, the neglect of which sometimes leads to error and inconclusive reasoning. It never can be too often repeated, that analysis is merely an engine of investigation, although a very powerful one. All its force and all its beauty are derived, as in the ancient geometry, from the certainty of the principles on which it proceeds, and from the clearness with which it traces their consequences. The matter we are considering will furnish an example of a theory which is correct in general, but which becomes defective when applied in circumstances where a modification is necessary. In the foregoing investigation p is the gravity at the level surface immediately below the external surface; or rather, it is the gravity which, according to the principles of the differential calculus, is supposed to remain without change from the one surface to the other. The force p therefore depends entirely on the level surface and the matter within it; $p \times k \times ds$ is the pressure which this force causes by its action on an elementary part of the superincumbent stratum. In the investigation of Clairaut the pressure mentioned is supposed to be the only force which the stratum exerts on the fluid below it; and if we allow that it is equable, and likewise that the whole mass is *in equilibrio*, it will follow that the part bounded by the level surface will be *in equilibrio* separately, if the stratum above it were taken away or annihilated. But, in the case of a planet, there is another force that must be taken into account, besides the pressure of the stratum caused by the gravitation at the level surface: it is the attraction of the stratum itself upon all the particles within it. The existence of this force is a consequence of the law universally prevailing in nature, that every particle of matter attracts every other particle. Thus, in the case of a planet, the stratum is made to press upon the fluid below it by two different forces,—by the gravitation at the level surface, and by the attraction which its own matter exerts upon the particles within it. Both these pressures must be equable over the whole level surface, otherwise Clairaut's reasoning will not apply; and as they are produced by independent causes, they cannot possibly be included in one and the same equation. In the case of a planet it therefore becomes necessary to add to Clairaut's theory a new condition, which can only be derived from the figure of the stratum. If we suppose that the stratum is possessed of such a figure as to attract every particle in the inside with equal force in opposite directions, it is manifest that the attraction of the stratum will not disturb the equilibrium of the fluid below it, and consequently that the pressure upon the level surface must be equable. Every level stratum being subjected to this new condition at the same time that the usual equation of the level surface is retained, we may extend Clairaut's demonstration to the case of a planet; and as the two conditions are sufficient, so we must infer that they are necessary, for the equilibrium. Thus are we led to the true conditions requisite to the equilibrium of a homogeneous mass of fluid consisting of particles that mutually attract one another, which are these two; viz. 1st, The resultant of the forces acting at every point of the external surface must be perpendicular to that surface; 2dly, A stratum of the fluid contained between two level surfaces must attract every particle in the inside with equal forces in opposite directions.

It follows from what has been proved, that the solution of Laplace is defective, because one of the conditions that must be attended to in the case of a planet is omitted. We have no direct evidence that the figure brought out is one of equilibrium. Whether the result be exact or not, is accidental, and can be known in no other way than by a comparison with other solutions derived from unexceptionable principles.

We can now assign the reason why Maclaurin, and all those who supposed an elliptical spheroid, have succeeded in this investigation. That condition of equilibrium which has always been omitted in the hydrostatical theory, is contained in the assumed figure. In a homogeneous ellipsoid the level surfaces are similar to the outer surface; and the author of the *Principia* has proved that a hollow shell of homogeneous matter, contained between two similar elliptical surfaces, attracts a particle in the inside with equal force in opposite directions. Thus the most difficult and abstruse part of the investigation being contained in the very hypothesis assumed, the rest of the solution is readily completed by the more obvious principles of hydrostatics. It deserves to be mentioned that of the two requisite conditions, the second, or the one which has always been omitted, determines the kind of figure without which the equilibrium cannot subsist; the other ascertains the proportion of the forces necessary to make that figure possible.

We can now likewise discover, *à priori*, why Laplace's method, in the circumstances supposed, brings out a result which is exact as a first approximation. It arises from this,—that, when the fluid is a perfect sphere, the two conditions of equilibrium coincide in one; for Newton has proved that a spherical shell of homogeneous matter attracts a particle in the inside equally in opposite directions. What is exactly true in the case of a sphere, must be nearly so when the fluid approaches indefinitely to that figure; and in these circumstances, the second condition being implied to a certain extent in the first, we obtain an approximation by means of the latter alone. This, however, is true only of the first step in approximating to the figure of a planet; in proceeding further it becomes necessary to take in all the conditions of equilibrium.

I shall only add one more remark on the analysis of Laplace. The radius of the spheroid, drawn from the centre of gravity, is represented by a series of this form, viz.

$$r = a + \alpha a \{Y^{(2)} + Y^{(3)} + Y^{(4)} + \&c.\},$$

the terms on the right-hand side being the development of the function y. This expression has nothing to do with the conditions of equilibrium; it belongs to every spheroid nearly spherical. Now the second of the conditions of the problem, or that one which determines the species of the figure necessary to the equilibrium, proves immediately that the radius can consist only of two terms, viz.

$$r = a + \alpha a Y^{(2)}.$$

In the method of Laplace the superfluous terms are got rid of by a particular mode of reasoning, which does not cohere well with the rest of the investigation, and certainly hurts the unity of solution. The process, indeed, is not long in the case of a uniform density; but when the fluid is composed of strata of variable density, the reasoning is both long and complicated, and the result is at length brought out by dint of calculation *. There is therefore a great advantage in solving the problem, as it ought to be solved, by the true principles of the case, both because it is more satisfactory to the mind, and because it is more simple. The observation here made is of the greater importance as it extends to the theory of the tides, and to the other questions treated of in the *Mécanique Céleste*, where the figure of the planets is concerned, the same form of the radius being assumed in all these cases.

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* Livre iii. § 29 & 30.

In a paper printed in the Philosophical Transactions for 1824 I have investigated the conditions of the equilibrium of a homogeneous mass of fluid, and have applied them to determine the figure which it will assume when it is urged by the attraction of its particles and a centrifugal force caused by a rotation about an axis passing through the centre of gravity. This is the first general solution of the problem that has been deduced from the principles of hydrostatics, without having recourse to approximations, and without introducing arbitrary suppositions. It must be allowed, however, that the investigation is in some respects not so simple as it might be made. But I propose to return to this subject, and in a particular work to treat the theory of the figure of the planets from its fundamental principles.

Feb. 3, 1826. JAMES IVORY.

N. B. In the *Conn. des Tems.* for 1828, M. Puissant, in a note at p. 220, notices a mistake in *naming* an angle I fell into in the solution of a geodetical problem inserted in the Philosophical Magazine for July 1824, and which is corrected in the same Journal for April 1825. The least attention to the solution would have shown M. Puissant that the import of the angle, which I have inadvertently called the *true latitude*, is fixed by the assumed values of the co-ordinates. Hence it cannot possibly be what I have said it is; it is the *reduced latitude*, and can be nothing else. There certainly is a misnomer; but the accuracy of the solution is not affected by it, because the meaning of the angle is determined independently of the name given to it. M. Puissant uses these words, "*Il est aisé de voir que le calcul n'est pas fondé sur des considérations analytiques assez rigoureuses, puisque le theorème qui en découle n'est pas parfaitement exact.*" Now here M. Puissant is passing sentence without having examined the case. My solution is perfectly exact: it is deduced from the most rigorous principles of analysis; although, in enunciating the theorem in question, I have inadvertently given the name of the true latitude to an angle, which in the analysis stands for the reduced latitude, and can possibly stand for nothing else. In the pages of the same Journal I have made many observations relating to subjects treated of in the *Conn. des Tems;* and although it would excite no surprise to find all these discussions passed by in silence in that work, yet it is rather remarkable that an inadvertence alone, substantially of no moment, and which has been corrected so long ago, is held up to public notice and carelessly misrepresented. J. I.

XIV. *Further Researches on the Preservation of Metals by Electro-chemical Means.* *By Sir* Humphry Davy, *Bart. Pres. R.S.*[*]

IN two papers read before the Royal Society I have described the effects of small quantities of electro-positive metals in preventing the corrosion or chemical changes of copper exposed to sea water, and I have stated that the results appear to be of the same kind, whether the experiments are made upon a minute scale and in confined portions of water, or on large masses and in the ocean.

The first and preliminary experiments proved that the copper sheeting of ships might be preserved by this method; but another and a no less important circumstance was to be attended to,—how far the cleanness of the bottom, or its freedom from the adhesion of weeds or shell fish, would be influenced by this preservation.

The use of the copper sheathing on the bottom of ships is twofold: First, to protect the wood from destruction by worms:

And secondly, to prevent the adhesion of weeds, barnacles, and other shell fish. No worms can penetrate the wood as long as the surface of the copper remains perfect; but when copper has been applied to the bottom of a ship for a certain time, a green coating or rust, consisting of oxide, submuriate and carbonate of copper, and carbonate of magnesia, forms upon it, to which weeds and shell fish adhere.

As long as the whole surface of the copper changes or corrodes, no such adhesions can occur; but when this green rust has partially formed, the copper below is protected by it, and there is an unequal action produced, the electrical effect of the oxide, submuriate, and carbonate of copper formed, being to produce a more rapid corrosion of the parts still exposed to sea water; so that the sheets are often found perforated with holes in one part after being used five or six years, and comparatively sound in other parts.

There is nothing in the poisonous nature of the metal which prevents these adhesions. It is *the solution* by which they are *prevented—the wear* of surface. Weeds and shell fish readily adhere to the poisonous salts of lead which form upon the lead protecting the fore part of the keel; and to the copper, in any chemical combination in which it is insoluble.

In general, in ships in the navy the first effect of the adhesion of weeds is perceived upon the heads of the mixed metal nails, which consist of copper alloyed by a small quantity of tin. The oxides of tin and copper which form upon the head of the nail and in the space round it, defend the metal from the action of sea water; and being negative with respect to it, a stronger corroding effect is produced in its immediate vicinity, so that the copper is often worn into deep and irregular cavities in these parts.

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[*] From the Philosophical Transactions for 1825, part ii.

When copper is unequally worn, likewise in harbours or seas where the water is loaded with mud or mechanical deposits, this mud or these deposits rest in the rough parts or depressions in the copper, and in the parts where the different sheets join, and afford a soil or bed in which sea weeds can fix their roots, and to which zoophytes and shell fish can adhere.

As far as my experiments have gone, small quantities of other metals, such as iron, tin, zinc, or arsenic, in alloy in copper, have appeared to promote the formation of an insoluble compound on the surface; and consequently there is much reason to believe must be favourable to the adhesion of weeds and insects.

I have referred in my last paper to the circumstance of the carbonate of lime and magnesia forming upon sheets of copper, protected by a quantity of iron above 1-120th part, when these sheets were in harbour and at rest.

The various experiments that I have caused to be made at Portsmouth show all the circumstances of this kind of action; and I have likewise elucidated them by experiments made on a smaller scale, and in limited quantities of water. It appears from these experiments that sheets of copper, at rest in sea water, always increase in weight from the deposition of the alkaline and earthy substances when defended by a quantity of cast iron under 1-150th of their surface; and if in a limited or confined quantity of water, when the proportion of the defending metal is under 1-4000th. With quantities below these respectively proportional for the sea, and limited quantities of water, the copper corrodes; at first it slightly increases in weight, and then slowly loses weight. Thus a sheet of copper 4 feet long, 14 inches wide, and weighing 9lbs. 6oz., protected by 1-100th of its surface of cast iron, gained in ten weeks and five days 12 drachms, and was coated over with carbonate of lime and magnesia: a sheet of copper of the same size protected by 1-150, gained only 1 drachm in the same time, and a part of it was green from the adhering salts of copper; whilst an unprotected sheet of the same class, both as to size and weight, and exposed for the same time, and as nearly as possible under the same circumstances, had lost 14 drachms: but experiments of this kind, though they

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* See Phil. Mag. vol. lxv. p. 205.

agree when carried on under precisely similar circumstances, must of necessity be very irregular in their results, when made in different seas and situations, being influenced by the degree of saltness, and the nature of the impregnations of the water, the strength of tide and of the waves, the temperature, &c.

In examining sheets which had been defended by small quantities of iron in proportions under 1-250 and above 1-1000, whether they were exposed alone, or on the sides of boats, there seemed to me no adhesions of *Confervæ*, except in cases where the oxide of iron covered the copper immediately round the protectors; and even in these instances such adhesions were extremely trifling, and might be considered rather as the vegetations caught by the rough surface of the oxide of iron, than as actually growing upon it.

Till the month of July 1824 all the experiments had been tried in harbour, and in comparatively still water; and though it could hardly be doubted that the same principles would prevail in cases where ships were in motion, and on the ocean, yet still it was desirable to determine this by direct experiment; and I took the opportunity of an expedition intended to ascertain some points of longitude in the North Seas, and which afforded me the use of a steam-boat, to make these researches. Sheets of copper carefully weighed, and with different quantities of protecting metal, and some unprotected, were exposed upon canvass so as to be electrically insulated upon the bow of the steam-boat; and were weighed and examined at different periods, after being exposed in the North Seas to the action of the water during the most rapid motion of the vessel. Very rough weather interfered with some of these experiments, and many of the sheets were lost, and the protectors of others were washed away; but the general results were as satisfactory as if the whole series of the arrangements had been complete. It was found that undefended sheets of copper of a foot square lost about 6·55 grains in passing at a rate averaging that of eight miles an hour in twelve hours; but a sheet having the same surface, defended by rather less than 1-500, lost 5·5 grains; and that like sheets defended by 1-70th and 1-100th of malleable iron were similarly worn, and underwent nearly the same loss, that of two grains, in passing through the same space of water. These experiments (the results of which were confirmed by those of others made during the whole of a voyage to and from Heligoland, but in which during the return the protectors were lost) show that motion does not affect the nature of the limits and quantity of the protecting metal; and likewise prove that, independently of the chemical, there is a mechanical wear of the copper in

sailing,

sailing, and which on the most exposed part of the ship, and in the most rapid course, bears a relation to it of nearly 2 to 4·55.

I used the very delicate balance belonging to the Royal Society in these experiments; the sheets of copper weighed between 7 and 8000 grains; and I was fully enabled to ascertain by means of this balance a diminution of weight upon so large a quantity, equal to 1-100th of a grain. It was evident from a very minute inspection of the sheet with the largest quantity of protecting metal, that there was not any adhesion of alkaline or earthy substances to its surface.

Having observed, in examining the results of some of the experiments on the effects of single masses of protecting metal on the sheeting of ships, that there was in some cases in which sheets with old fastening had been used, tarnish or corrosion, which seemed to increase with the distance from the protecting metal, it became necessary to investigate this circumstance, and to ascertain the extent of the diminution of electrical action in instances of imperfect or irregular conducting surfaces.

With single sheets or wires of copper, and in small confined quantities of sea water, there seemed to be no indications of diminution of conducting power, or of the preservative effects of zinc or iron, however divided or diffused the surface of the copper, provided there was a perfect metallic connexion through the mass. Thus a small piece of copper, containing about 32 square inches, was perfectly protected by a quantity of zinc which was less than 1-4000th part of the whole surface; and a copper wire of several feet in length was prevented from tarnishing by a piece of zinc wire which was less than 1-1400th part of its length. In these cases the protecting metal corroded with great rapidity, and in a few hours was entirely destroyed; but when applied in the form of wire and covered, except at its transverse surface, with cement, its protecting influence upon the same minute scale was exhibited for many days. A part of these results depend upon the absorption of the oxygen dissolved in the water when its quantity is limited, by the oxidable metal, and of course the proportion of this metal must be much larger when the water is constantly changing; but the experiments seem to show that any diminution of protecting effect at a distance does not depend upon the nature of the metallic, but of the imperfect or fluid conductor.

This indeed is shown by many other results.

A piece of zinc and a piece of copper in the same vessel of sea water, but not in contact, were connected by different lengths of fine silver wire of different thickness. It was found that whatever lengths of wire of 1-300th of an inch were used, there was no diminution of the protecting effect of the zinc; and the experiment was carried so far as to employ the whole of a quantity of extremely fine wire, amounting to upwards of forty feet in length, and of a diameter equal only to 100-98742 of an inch, when the results were precisely the same as if the zinc and copper had been in immediate contact.

Pieces of charcoal, which is the worst amongst the more perfect conductors, were connected by being tied together, and made the medium of communication between zinc and copper, upon the same principles, and with the same views as those just described, and with precisely the same consequences.

In my first experiments upon the effects of increasing the length or diminishing the mass of the imperfect or fluid conducting surface in interfering with the preserving effects of metals, I used long narrow tubes; but I found them very inconvenient; and I had recourse to the more simple method of employing cotton or tow for this purpose.

Several feet of copper wire in a spiral form were connected with a small piece of zinc wire of about half an inch in length. The zinc and a portion of the copper were introduced into one glass, and the coils of copper wire were introduced into other glasses, so as to form a series of six or seven glasses, which were filled with sea water, and made part of the same voltaic arrangement, by being connected with pieces of tow moistened in sea water.

It was found in these experiments, that when the pieces of tow connecting the glasses were half an inch in thickness, the preserving effect of the zinc in the first glass was no where diminished, but extended apparently equally through the whole series.

When the pieces of tow were about the fifth of an inch in thickness, a diminution of the preserving effects of the zinc was perceived in the fourth glass, in which there was a slight solution of copper; in the fifth glass this result was still more distinct, and so on till in the seventh glass there was a considerable corrosion of the copper.

When the tow was only the tenth of an inch in thickness, the preserving effect of the zinc extended only to the third glass; and in each glass more remote, the effect of corrosion was more distinct, till in the seventh glass it was nearly the same as if there had been no protecting metal. All the chemical changes dependent upon negative electricity were successively and elegantly exhibited in this experiment. In the first glass, containing the zinc, there was a considerable and hasty deposition of earthy and alkaline matter, and crystals of carbonate of soda adhered to the copper at the surface where it was clean and bright; but in the lower part it was coated with revived metallic zinc. In the second glass the wire was covered over with fine crystals of carbonate of lime; and the same phænomenon of the separation of carbonate of soda occurred, but in a less degree. In the third glass the wire was clean, but without depositions; and the presence of alkaline matter could only be distinguished by chemical tests. In the fourth glass the copper was bright, evidently in consequence of a slight but general corrosion, but with a scarcely sensible deposit; in the fifth, the deposit was very visible; and in the seventh the wire was covered with green rust.

These results, which showed that a very small quantity only of the imperfect or fluid conductor was sufficient to transmit the electrical power, or to complete the chain, induced me to try if copper nailed upon wood, and protected merely by zinc or iron on the under surface, or that next the wood, would not be defended from corrosion. For this purpose I covered a piece of wood with small sheets of copper, a nail of zinc of about the 1-200th part of the surface of the copper being previously driven into the wood: the apparatus was plunged in a large jar of sea water: it remained perfectly bright for many weeks; and when examined, it was found that the zinc had only suffered partial corrosion, that the wood was moist, and that on the interior of the copper there was a considerable portion of revived zinc, so that the negative electricity, by its operation, provided materials for its future and constant excitement. In several trials of the same kind, iron was used with the same results; and in all these experiments there appeared to be this peculiarity in the appearance of the copper, that unless the protecting metal below was in very large mass, there were no depositions of calcareous or magnesian earths upon the metal; it was clean and bright, but never coated. The copper in these experiments was nailed sometimes upon paper, sometimes upon the mere wood, and sometimes upon linen; and the communication was partially interrupted between the external surface and the internal surface by cement; but even one side or junction of a sheet seemed to allow sufficient communication between the moisture on the under surface and the sea water without, to produce the electrical effect of preservation.

These results upon perfect and imperfect conductors led to another inquiry, important as it relates to the practical application of the principle; namely, as to the extent and nature of the contact or relation between the copper and the preserving metal. I could not produce any protecting action of zinc or iron upon copper through the thinnest stratum of air, or the finest leaf of mica, or of dry paper; but the action of the metals did not seem to be much impaired by the ordinary coating of oxide or rust; nor was it destroyed when the finest bibulous or silver paper, as it is commonly called, was between them, being moistened with sea water. I made an experiment with different folds of this paper. Pieces of copper were covered with one, two, three, four, five and six folds; and over them were placed pieces of zinc, which were fastened closely to them by thread: each piece of copper so protected was exposed in a vessel of sea water, so that the folds of paper were all moist.

It was found, in the case in which a single leaf of paper was between the zinc and the copper, there was no corrosion of the copper; in the case in which there were two leaves, there was a very slight effect; with three, the corrosion was distinct; and it increased, till with the six folds the protecting power appeared to be lost: and in the case of the single leaf there was this difference from the result of immediate contact, that there was no deposition of earthy matter:—showing that there was no absolute minute contact of the metals through the moist paper; which was likewise proved by other experiments: for a thin plate of mica, as I have just mentioned, entirely destroyed the protecting effect of zinc; and yet when a hole was made in it, so as to admit a very thin layer of moisture between the zinc and copper, the corrosion of the copper, though not destroyed, was considerably diminished.

The rapid corrosion of iron and zinc, particularly when used to protect metals, only in very small quantities, induced me to try some experiments as to their electro-chemical powers in menstrua out of the contact, or to a certain extent removed from the contact of air, such as might be used for moistening paper under the copper sheathing of ships. The results of these experiments I shall now detail. A small piece of iron was placed in one glass filled with a saturated solution of brine, which contains little or no air; copper, attached by a wire to the iron, was placed in a vessel containing sea water, which was connected with the brine by moistened tow. The copper did not corrode, and yet the iron was scarcely sensibly acted upon, and that only at the surface of the brine; and a much less effect was produced upon it in many weeks than would have been occasioned by sea water in as many days.

With zinc and brine in the same kind of connexion there was a similar result; but the solution of the zinc was comparatively more rapid than that of the iron, and the copper was rendered more highly negative, as was shown by a slight deposition of earthy matter upon it.

A solution of potassa, or of alkaline substances possessing the electro-positive energy, has nearly the same effect on saline solutions as if they were deprived of air, and when mixed with sea water impedes the action of metals upon them; but if used in quantity in combinations such as these I have just described, in which iron is the protecting metal, it destroys the result, and renders the iron negative. Thus, if iron and copper in contact, or fastened to each other by wires, be in two vessels of sea water connected by moist cotton or asbestos, all the various circumstances of protection of the two metals by each other may be exhibited by means of solution of potassa. By adding a few drops of solution of potassa to the water in the glass containing the iron, the negative powers of the copper in the other glass are diminished; so that the deposition of the calcareous and magnesian earths upon it is considerably lessened: by a little more solution of potassa the deposition is destroyed, but still the copper remains clean. The corrosion of the iron, which before was rapid, is now almost at an end; and a few drops more of the solution of potassa produces a perfect equilibrium: so that neither of the metals undergoes any change, and the whole system is in a state of perfect repose. By making the fluid in the glass containing the iron still more alkaline, it no longer corrodes; and the green tint of the sea water shows that the copper is now the positively electrified metal; and when the solution in the glass containing the iron is strongly alkaline, the copper in the other glass corrodes with great rapidity, and the iron remains in the electro-negative and indestructible state.

I began this paper by some observations upon the nature of the process by which copper sheeting is destroyed by sea water, and on the causes by which it is preserved clean, or rendered foul by adhesions of marine vegetables or animals; I shall conclude it by some further remarks on the same subject, and with some practical inferences and some theoretical elucidations, which naturally arise from the results detailed in the foregoing pages.

The very first experiment that I made on harbour-boats at Portsmouth, proved that a single mass of iron protected fully and entirely many sheets of copper, whether in waves, tides, or currents, so as to make them negatively electrical, and in such a degree as to occasion the deposition of earthy matter upon them: but observations on the effects of the single contact of iron upon a number of sheets of copper, where the junctions and nails were covered with rust, and that had been in a ship for some years, showed that the action was weakened in the case of imperfect connexions by distance, and that the sheets near the protector were more defended than those remote from it. Upon this idea I proposed, that when ships, of which the copper sheathing was old and worn, were to be protected, a greater proportion of iron should be used, and that if possible it should be more distributed. The first experiment of this kind was tried on the Sa[illegible] of 28 guns, in March 1824, and which had been coppered [illegible] years before in India. Cast iron, equal in surface to about 1-80th of that of the copper, was applied in four masses, two near the stern, two on the bows. She made a voyage to Nova Scotia, and returned in January 1825. A false and entirely unfounded statement respecting this vessel was published in most of the newspapers,—that the bottom was covered with weeds and barnacles. I was at Portsmouth soon after she was brought into dock: there was not the smallest weed or shell-fish upon the whole of the bottom from a few feet round the stern protectors to the lead on her bow. Round the stern protectors there was a slight adhesion of rust of iron, and upon this there were some zoophytes of the capillary kind, of an inch and a half or two inches in length, and a number of minute barnacles, both *Lepas anatifera* and *Balanus Tintinnabulum.* For a considerable space round the protectors, both on the stern and bow, the copper was bright; but the colour became green towards the central parts of the ship; yet even here the rust or verdigrease was a light powder, and only small in quantity, and did not adhere, or come off in scales, and there had been evidently little copper lost in the voyage. That the protectors had not been the cause of the trifling and perfectly insignificant adhesions by any electrical effect, or by occasioning any deposition of earthy matter upon the copper, was evident from this,—that the lead on the bow, the part of the ship most exposed to the friction of the water, contained these adhesions in a much more accumulated state than that in which they existed near the stern; and there were none at all on the clean copper round the protectors in the bow; and the slight coating of oxide of iron seems to have been the cause of their appearance.

I had seen this ship come into dock in the spring of 1824, before she was protected, covered with thick green carbonate and submuriate of copper, and with a number of long weeds, principally *Fuci*, and a quantity of zoophytes, adhering to different parts of the bottom; so that this first experiment was

highly

highly satisfactory, though made under very unfavourable circumstances.

The only two instances of vessels which have been recently coppered, and which have made voyages furnished with protectors, that I have had an opportunity of examining, are the Elizabeth yacht, belonging to the Earl of Darnley, and the Carnebrea Castle, an Indiaman, belonging to Messrs. Wigram. The yacht was protected by about 1-125th part of malleable iron placed in two masses in the stern. She had been occasionally employed in sailing, and had been sometimes in harbour, during six months. When I saw her in November she was perfectly clean, and the copper apparently untouched. Lord Darnley informed me that there never had been the slightest adhesion of either weed or shell-fish to her copper, but that a few small barnacles had once appeared on the loose oxide of iron in the neighbourhood of the protectors, which however were immediately and easily washed off. The Carnebrea Castle, a large vessel of upwards of 650 tons, was furnished with four protectors, two on the stern and two on the bow, equal together to about 1-104th of the surface of the copper. She had been protected more than twelve months, and had made the voyage to Calcutta and back. She came into the river perfectly bright; and when examined in the dry dock was found entirely free from any adhesion, and offered a beautiful and almost polished surface; and there seemed to be no greater wear of copper than could be accounted for from mechanical causes.

Had these vessels been at rest, I have no doubt there would have been adhesions, at least in Portsmouth or Sheerness harbours, where the water is constantly muddy, and where the smallest irregularity or roughness of surface, from either wear, or the deposition of calcareous matter, or the formation of oxides or carbonates, enables the solid matter floating in the water to rest. There is a ship, the Howe, one of the largest in the navy, now lying at Sheerness, which was protected by a quantity of cast iron judged sufficient to save all her copper, nearly fifteen months ago. She has not been examined; but I expect and hope that the bottom will be covered with adhesions, which must be the case if her copper is not corroded: but notwithstanding this, whenever she is wanted for sea, it will only be necessary to put her into dock for a day or two, scrape her copper, and wash it with a small quantity of acidulous water, and she will be in the same state as if newly coppered.

At Liverpool, as I am informed, several ships have been protected, and have returned after voyages to the West Indies, and even to the East Indies. The proportion of protecting metal in all of them has been beyond what I have recommended, 1-90th to 1-70th; yet two of them have been found perfectly clean, and with the copper untouched after voyages to Demarara; and another nearly in the same state, after two voyages to the same place. Two others have had their bottoms more or less covered with barnacles; but the preservation of the copper has been in all cases judged complete. The iron has been placed along the keel on both sides; and the barnacles, in cases where they have existed, have been generally upon the flat of the bottom; from which it may be concluded, that they adhered either to the oxide of iron, or the calcareous deposits occasioned by the excess of negative electricity.

In the navy the proportion adopted has been only 1-250th of cast iron, at least for vessels in actual service, and when the object is more cleanness than the preservation of the copper.

In is very difficult to point out the circumstances which have rendered results, such as these mentioned with respect to Liverpool traders, so different under apparently the same circumstances, *i. e.* why ships should exhibit no adhesions or barnacles after two voyages, whilst on another ship, with the same quantity of protection, they should be found after a single voyage*. This may probably depend upon one ship having remained at rest in harbour longer than another, or having been becalmed for a short time in shallow seas, where ova of shell fish, or young shell fish existed; or upon oxide of iron being formed, and not washed off, in consequence of calm weather, and which consolidating, was not afterwards separated in the voyage. From what I can learn, however, the chance of a certain degree of foulness, in consequence of the application of the full proportion of protecting metal, will not prevent ship-owners from employing this proportion, as the saving of copper is a very great object; and as long as the copper is sound, no danger is to be apprehended from worms.

It ought to be kept in mind that the larger a ship, the more the experiment is influenced by the imperfect conducting power of the sea water, and consequently the proportion of protecting metal may be larger without being in excess.

I have mentioned these circumstances because they apply to ships already coppered, and because I have heard that a Liverpool ship, of which it was doubtful whether the copper was in a state such as would enable her to make another voyage to India with security, has, by the application of protectors of 1-70th, made this voyage *, without apparently any wear of her sheeting; and that she is now preparing with the same protectors to make another voyage.

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* The quality of the copper may be another cause.

In cases when ships are to be newly sheathed, the experiments which have been detailed in the preceding pages render it likely, that the most advantageous way of applying protection will be under, and not over the copper: the electrical circuit being made in the sea water passing through the places of junction in the sheets; and in this way every sheet of copper may be provided with nails of iron or zinc, for protecting them to any extent required. By driving the nail into the wood through paper wetted with brine *above* the tarred paper, or felt, or any other substance that may be employed, the incipient action will be diminished; and there is this great advantage, that a considerable part of the metal will, if the protectors are placed in the centre of the sheet, be deposited and re-dissolved: so there is reason to believe that small masses of metal will act for a great length of time. Zinc, in consequence of its forming little or no insoluble compound in brine or sea water, will be preferable to iron for this purpose; and whether this metal or iron be used, the waste will be much less than if the metal was exposed on the outside: and all difficulties with respect to a proper situation in this last case are avoided.

The copper used for sheathing should be the purest that can be obtained; and in being applied to the ship, its surface should be preserved as smooth and equable as possible: and the nails used for fastening should likewise be of pure copper; and a little difference in their thickness and shape will easily compensate for their want of hardness.

In vessels employed for steam navigation the protecting metal can scarcely be in excess†, as the rapid motion of these ships prevent the chance of any adhesions; and the wear of the copper by proper protection is diminished more than two-thirds.

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* The Dorothy.

† I have mentioned in the two last communications on this subject some application of the principle; many others will occur. In submarine constructions—to protect wood, as in piles, from the action of worms, sheathing of copper defended by iron in excess may be used: when the calcareous matter deposited will gradually form a coating of the character and firmness of hard stone.

XV. *Reply to Mr.* DAVIES'S *Postscript on Mr.* HERAPATH'S *Demonstration. By* P. Q.

To the Editor of the Philosophical Magazine and Journal.

Sir,

I BEG to trouble you with one more view of the extremely simple and obvious matter of dispute between Mr. Davies and myself; and if this be not satisfactory to Mr. D. I must despair of giving any that is, and shall therefore give up the point.

Let x, y be any two variable and absolutely independent integers; and let a be any non-integer also independent of both x and y. Then

$$(x + a) + (y - a) = z = \text{some variable integer.}$$

But because x is a variable integer independent of y, and a is independent of both, $x + a$ considered as a variable, whose changes are by integral saltations, is independent of $y - a$, whose changes are likewise by integral saltations. That is, the saltations of $x + a$ do not necessarily affect the value or saltations of $y - a$; which is evidently the utmost that the principle of Mr. Herapath's demonstration requires; for it only needs that the function of one non-integer, of $x + a$ for example, should not be affected by the changes of the other, $y - a$. Mr. Davies's error seems to consist in making the variation of these non-integers continuous and to depend on that of the common part a.

I wish to make no observations that may provoke a reply; but I beg to observe, that Mr. Davies is altogether mistaken in saying, "that the inquiry does not call for the consideration of indeterminate *integers*." The spirit of Mr. Herapath's demonstration (Phil. Mag. for May 1825, p. 324,) is expressly founded on the assumed indeterminate nature of the integer n, as any one may see by referring to the above page.

If Mr. Davies will also turn to the page he refers to, "Phil. Mag., November 1824, p. 333," and also to Annals of Philos. for December 1824, p. 420, he will find that "Mr. Herapath's mode of establishing some theorems in periodical functions," is *not* "founded on the assumed independence of two fractions whose sum is an integer;" nor has it the most distant allusion to such a principle. Of course, Mr. Davies's objections, which he admits are grounded on such a presumption, rest on mistaken views.

I have already observed, that I wish not to say anything which may educe a reply; therefore, and therefore only, I pass over Mr. Davies's defence of his circular comparison, and one or two other points.

In retiring from this controversy, I do it with a full and unqualified conviction of the perfect accuracy and completeness of that part of Mr. Herapath's labours I have endeavoured to defend. I retire, because I am sure the further occupying of your pages with a subject so simple and axiomatic, will appear to the majority of your readers unnecessary and superfluous. With respect to Mr. Davies, I leave him in full possession of my esteem, and on any other subject I shall be most happy to see your Journal ornamented with his labours.

P. Q.

ERRATUM in my paper, Phil. Mag., Nov. 1825.

Page 355, Theorem, *for* $F_3(p_r, q_r)$ *read* $F_3(p_1, q_v)$.

XVI. *Notice of a Meteoric Stone which fell at Nanjemoy, in Maryland, North America, on February* 10, 1825. *By Dr.* SAMUEL D. CARVER. *In a Letter to Professor* SILLIMAN[*].

I TAKE the liberty of forwarding you a notice of a meteoric stone which fell in this town on the morning of Thursday, February 10, 1825. The sky was rather hazy, and the wind south-west. At about noon the people of the town and of the adjacent country were alarmed by an explosion of some body in the air, which was succeeded by a loud whizzing noise, like that of air rushing through a small aperture, passing rapidly in the course from north-west to south-east, nearly parallel with the river Potomac. Shortly after, a spot of ground on the plantation of Capt. W. D. Harrison, surveyor of this port, was found to have been recently broken; and on examination a rough stone of an oblong shape, weighing sixteen pounds and seven ounces, was found about eighteen inches under the surface. The stone, when taken from the ground, about half an hour after it is supposed to have fallen, was sensibly warm, and had a strong sulphureous smell. It has a hard vitreous surface, and when broken appears composed of an earthy or siliceous matrix, of a light slate colour, containing numerous globules of various sizes, very hard, and of a brown colour, together with small portions of brownish yellow pyrites, which become dark coloured on being reduced to powder. I have procured for you a *fragment*[†] of the stone, weighing *four pounds* and *ten ounces*, which was all I could obtain. Various notions were entertained by the people in the neighbourhood on finding the stone. Some supposed it propelled from a quarry eight or ten miles distant on the opposite side of the river; while others thought it thrown by a mortar from a packet lying at anchor in the river, and even proposed manning boats to take vengeance on the captain and crew of the vessel.

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[*] From Silliman's Journal of Science, vol. ix. p. 351.

[†] This specimen is not yet received.—AMER. EDIT.

I have conversed with many persons living over an extent of perhaps fifty miles square; some heard the explosion, while others heard only the subsequent whizzing noise in the air. All agree in stating that the noise appeared directly over their heads. One gentleman, living about 25 miles from the place where the stone fell, says, that it caused his whole plantation to shake, which many supposed to be the effect of an earthquake. I cannot learn that a fire-ball or any light was seen in the heavens,—all are confident that there was but one report, and no peculiar smell in the air was noticed. I herewith transmit the statement of Capt. Harrison, the gentleman on whose plantation the stone fell.

Statement of W. D. Harrison, Esq.

On the 10th of February 1825, between the hours of twelve and one o'clock, as nearly as recollected, I heard an explosion, as I supposed, of a cannon, but somewhat sharper. I immediately advanced with a quick step about twenty paces, when my attention was arrested by a buzzing noise, resembling that of a humming bee, which increased to a much louder sound, something like a spinning-wheel, or a chimney on fire, and seemed directly over my head, and in a short time I heard something fall. The time which elapsed from my first hearing the explosion, to the falling, might have been fifteen seconds. I then went with some of my servants to find where it had fallen, but did not at first succeed (though, as I afterwards found, I had got as near as 30 yards to the spot); however, after a short interval, the place was found by my cook, who had (in the presence of a respectable white woman) dug down to it before I got there, and a stone was discovered from 22 to 24 inches under the surface, and which after being washed, weighed sixteen pounds, and which was no doubt the one which I had heard fall, as the mud was thrown in different directions from 13 to 16 steps. The day was perfectly clear, a little snow was then on the earth in some places, which had fallen the night previous. The stone when taken up had a strong sulphureous smell; and there were black streaks in the clay which appeared marked by the descent of the stone. I have conversed with gentlemen in different directions, some of them from 18 to 20 miles distant, who heard the noise (not the explosion). They inform me that it appeared directly over their heads. There was no fire-ball seen by me or others that I have heard. There was but one report, and but one stone fell, to my knowledge, and there was no peculiar smell in the air. It fell on my plantation, within 250 yards of my house, and within 100 of the habitation of the negroes.

I have given this statement to Dr. Carver, at his request, and which is as full as I could give at this distant day, from having thought but little of it since. Given this 28th day of April 1825.

W. D. Harrison,
Surveyor of the port of Nanjemoy, Maryland.

XVII. *Analysis of the Maryland Aërolite. By* George Chilton, *Lecturer on Chemistry, &c.*[*]

THE piece of Maryland aërolite[†] subjected to examination, weighed 228·30 grains in air, and lost 62·25 grains by immersion in water, at 60° temperature. Its specific gravity is therefore 3·66. The external crust was taken off, and the remainder powdered, not very finely, and separated into two parts by the magnet; 40 grains were obedient to the magnet, 25 of which were taken for examination. The same quantity was taken of the unmagnetical portion.

Examination of the unmagnetical *Portion of the Maryland Aërolite.*

Process 1.—The 25 grains were digested in dilute nitric acid; an undissolved part floated, which, together with the solution, was decanted from a heavier part, which remained at the bottom of the flask. To this last, muriatic acid was added, and digestion continued till every thing soluble was taken up. The two insoluble parts, managed in the usual way and carefully dried, weighed 15·87 grains. During exposure to a red heat, in a crucible, sulphur burnt off with its usual blue flame, and left siliceous earth which weighed 14·6 grains.

Process 2.—The acid solutions were mixed together and evaporated slowly to dryness; during which, portions of matter fell down, which, together with a portion left after treating the dry mass with water, weighed 0·7 gr. at the common temperature. On further examination they proved to be silica and oxide of iron. By estimation, 0·3 silica, and 0·2 oxide of iron; in the perfectly dried state.

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[*] From Silliman's Journal, vol. x. p. 131.

[†] A notice of the fall of this aërolite was published in our last number: [see the preceding article.—Ed.] For a more particular description of the stone, and for illustrative remarks respecting it, see the end of this paper. Amer. Edit.

Process 3.—Bi-carbonate of potash was added to the solution, which was heated a little. The precipitate was separated by the filter, washed and digested in pure potash. The caustic liquor, drawn off by the syphon, super-saturated with muriatic acid, and treated with carbonate of ammonia, yielded a precipitate which after ignition weighed 0·1 gr. It appeared to be alumina contaminated with oxide of iron.

Process 4.—The filtered solution, from which the first precipitate in the last process was separated, was boiled; a gray earth fell down in flocks. The addition of potash occasioned a further deposit. On heating, it changed to a cinnamon-brown colour; dilute sulphuric acid, added in excess, dissolved it, with the exception of a brown residue, which weighed after ignition 0·2 gr. Before the blowpipe, with borax and phosphoric salt, this brown matter yielded yellow beads—indicating nickel?

Process 5.—The sulphuric solution of the last process was evaporated to dryness, and heated further to drive off the excess of acid. On adding water, a *part* only dissolved: on adding more water, the whole dissolved, except a portion of a brown colour, which by solution in muriatic acid, and subsequent precipitation by ammonia, yielded oxide of iron weighing 0·2 gr.

Process 6.—The last watery solution was gently evaporated to a small compass; sulphate of lime fell down during the evaporation. On leaving it to exhalation in the open air, sulphate of magnesia crystallized. These crystals, together with the deposited sulphate of lime, were exposed to a dull red heat. The weight, while warm, was 9 grains. On adding a saturated solution of sulphate of lime, to dissolve out the sulphate of magnesia, a portion was left, which weighed after ignition 1·1 grain. This subtracted from the weight of the mixed sulphates, leaves for sulphate of magnesia 7·9 grains.

Process 7.—The precipitate (Process 3), which had been digested in pure potassa, was redissolved in muriatic acid. Ammonia added in excess, threw down oxide of iron, which after ignition weighed 3·9 grains.

Process 8.—The last ammoniacal solution, which had a blueish green colour, was evaporated to dryness. After the further application of heat, to volatilize the ammoniacal salt, a residue was left of a dark-brown colour, which, on solution in nitric acid and precipitation by potassa, give a bulky apple-green precipitate, which turned to a dark-brown by heating it to ignition. It weighed 0·3 gr.

Process 9.—The liquor, from which the apple-green precipitate had been separated, had a wine-yellow colour, thereby affording a suspicion that it contained more metal. Neutralization and heat were both tried without effecting a further separation. Hydro-sulphuret of ammonia threw down a black precipitate. This precipitate heated, redissolved in nitric acid, and precipitated by potash, gave another apple-green precipitate, which ignited, weighed 0·2 gr. The solution being still a little coloured, was again treated with hydrosulphuret of ammonia, redissolved in nitric acid, and precipitated by potash. By this treatment another precipitate was obtained, which weighed 0·1 grain.

Process 10.—Twenty grains of the same unmagnetical aërolite were mixed with an equal weight of nitre, and heated in a bright red heat. On dissolving out the matter of the crucible and neutralizing the solution, it neither produced a yellow with nitrate of lead, nor a red with nitrate of mercury—hence it contained *no chrome.*

From the 25 grains there were obtained by these processes,

14·6 + 0·3	= silica . . .	14·90
7·9 sulph. mag.	= magnesia . .	2·60
1·1 sulph. lime	= lime . . .	0·45
3·9 + 0·2 + 2·0 + 0·5	= oxide of iron .	6·15
0·2 + 0·3 + 0·2 + 0·1	= oxide of nickel	0·80
	sulphur . .	1·27
	alumina . .	0·05
		26·12

It would seem superfluous to remark, that the increase of weight in this, and the following analysis, must be accounted for from the change of condition of the iron with respect to oxygen.

Examination of the magnetical *Portion of the Maryland Aërolite.*

Process 1.—Twenty-five grains exposed to the action of nitro-muriatic acid left, by the usual management, 3 grains of silica, after ignition.

Process 2.—Ammonia, added to excess, threw down from the acid solution oxide of iron, which weighed, after ignition, 24 grains.

Process 3.—To the ammoniacal solution, which had a blueish-green tinge, potash was added. On the application of heat a portion of earthy matter precipitated, too trifling for examination. Hydro-sulphuret of ammonia threw down a black precipitate, which, heated, redissolved in nitric acid, and precipitated by potash, yielded an olive-green precipitate, which, ignited, weighed 1·70 gr. and had a light-brown colour.

a. Nitric acid added to this precipitate, did not dissolve the whole of it. Muriatic acid was added without effecting a complete solution. The mixture was heated and evaporated nearly to dryness. On standing till the next day it formed a gelatinous mass of a green colour. Water was then added, and the insoluble portion separated by the filter. It weighed 5 grains, and had a gray colour.

b. Ammonia was added to the nitro-muriatic solution (*a*) in excess, which re-produced the blueish green tinge. By evaporation to dryness, and exposure to a red heat for some time, the ammoniacal salts were volatilized, and a yellowish brown oxide left.

c. Before the blowpipe, with borax and phosphoric salt, beads were produced of a brown colour, and opaque when the oxide was in considerable proportion to the salt; but when diluted with more salt, blood-red globules formed, which changed on cooling, to hyacinth-red, and when entirely cold had a fine yellow, with, in some instances, a slightly reddish cast. The undissolved portion produced the same appearances nearly, but less distinctly. Regarding, therefore, the precipitate 1·70, in Process 3, as oxide of nickel contaminated with siliceous earth, perhaps 1·25 may be put down for oxide of nickel. We shall then have, as the result of analysis of the magnetic aërolite,

Oxide of iron	24·00
Oxide of nickel	1·25
Silica with other earthy matter	3·46
Sulphur a trace.	
	28·71

The presence of sulphur was indicated by the odour of sulphuretted hydrogen, on the first addition of the acid.

Additional Notice of the Physical Characters of the Maryland Aërolite. By Professor SILLIMAN.

As the visits of these extraordinary strangers to our planet are frequent, and their origin is not yet satisfactorily explained, it is obviously proper to register carefully all the facts respecting them; that thus we, or those who follow us, may by and by be in a condition to reason correctly respecting them.

We hastened to lay before our readers the account which we received of the fall of the Maryland aërolite; but as no specimen had then been received, it was not possible to give at that time either a description or an analysis.—Mr. Chilton has supplied the analysis.—We add the following notice of the appearance of the stone.

An excellent specimen, for which we are indebted to Dr. Samuel D. Carver, weighs four pounds five ounces. Its dimensions are seven inches by three and four: its form is that of an irregular ovoidal protuberance, nearly flat where it was detached from the larger mass, and bounded by irregular curves in the other parts of the surface. In all parts, except where it has been fractured, it is covered by the usual black vitreous coating, which in this case, especially when it is viewed by a magnifier, has more lustre than is common. This coating is severed by innumerable cracks running in every direction, and communicating with each other, so as to divide the surface into polygons resembling honeycomb or madrepore, and no undivided portion of the surface exceeds half an inch in diameter.

This circumstance is much less apparent upon the aërolites of Weston (1807), L'Aigle (1803), and Stannern in Moravia (1808): it appears to have arisen from the rapid cooling of the external vitreous crust after intense ignition. It is impossible to doubt that this crust is a result of great and sudden heat. In the Maryland aërolite it is not quite so thick as the back of a common penknife, and, as in that of Weston and Stannern, it is separated by a well defined line from the mass of the stone beneath. The mass of the stone is, on the fractured surface, of a light ash-gray colour, or perhaps more properly of a grayish white: it is very uniform in its appearance, and not marked by that strong contrast of dark and light gray spots, which is so conspicuous in the Weston meteorolite. The fractured surface of the Maryland stone is uneven and granular, harsh and dry to the touch, and it scratches window glass decidedly, but not with great energy. To the naked eye it presents very small glistening metallic points, and a few minute globular or ovoidal bodies scattered here and there, through the mass of the stone. With a magnifier all these appearances are of course much increased. The adhesion of the small parts of the stone is so feeble, that it falls to pieces with a slight blow, and exhibits an appearance almost like grains of sand. The metallic parts are conspicuous, but they are much less numerous than the earthy portions, which, when separated, are nearly white, and have a pretty high vitreous lustre, considerably resembling porcelain. They appear as if they had undergone an incipient vitrification, and as if they had been feebly agglutinated by a very intense heat. I cannot say that I observed in them, as M. Fleuriau de Bellevue did in the aërolites of Jonzac (*Journ. de Phys.* tome xcii. p. 136), appearances of crystallization; although it is possible there may have been an incipient process of that kind, especially as the small parts are translucent*. The Maryland stone is highly magnetic; pieces as large as peas are readily lifted by the magnet, and that instrument takes up a large proportion of the smaller fragments. The iron is metallic and perfectly malleable; although none of the pieces are larger than a pin's head, still they are readily extended by the hammer. The iron in the crust is glazed over, so that the eye does not perceive its metallic character; but the file instantly brightens the innumerable points which then break through the varnish of the crust, and give it a brilliant metallic lustre, at all the points where the file has uncovered the iron. The same is the fact with the Weston stone, and with that of L'Aigle, but not with that of Stannern in Moravia; specimens of all of which, and of the meteoric iron of Pallas, of Louisiana, and of Auvergne, are now before me. The aërolites of Jonzac and of Stannern, as stated by M. Bellevue, are the only ones hitherto discovered that do not contain native iron, and do not affect the magnet; still their analysis presents a good deal of iron, which is probably in the condition of oxide.

The iron in the metallic state is very conspicuous in the Weston stone, sometimes in pieces of two inches in length; and both in this stone and in that of Maryland it is often brilliant like the fracture of the meteoric iron of Pallas and of Louisiana.

In the analysis of the Weston stone published in 1808, I did not discover chrome, although it was afterwards announced by Mr. Warden. I have desired Mr. Chilton to re-analyse the Weston stone, and he has nearly completed the labour, the result of which may be given hereafter; but he writes that he has not been able to discover any chrome. I am not quite sure that I discover pyrites in the Maryland aërolite, although it is mentioned by Dr. Carver in his letter in the preceding volume.

October 4, 1825.

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* This vitreous appearance I believe has not been observed before (at least as far as appears in any account that I have seen). It seems to have resulted from intense heat; the same, doubtless, which covered the exterior with the black crust; and the difference of the two is probably to be ascribed to the one being covered and compressed, and to the other being on the outside.

XVIII. *Essay on the Gales experienced in the Atlantic States of North America. By* Robert Hare, *M.D. Professor of Chemistry in the University of Pennsylvannia*[*].

OF the gales experienced in the Atlantic States of North America, those from the north-east and north-west are by far the most influential: the one, remarkable for its dryness —the other, for its humidity. During a north-western gale, the sky, unless at its commencement, is always peculiarly clear, and not only water, but ice, evaporates rapidly. A north-east wind, when it approaches to the nature of a durable gale, is always accompanied by clouds, and usually by rain or snow. The object of the following essay, is to account for this striking diversity of character.

When, by a rise of temperature, the lower portions of a non-elastic fluid are rendered lighter than those which are above them, an exchange of position must ensue. The particles which were coldest at first, after their descent, becoming the warmest, resume their previous elevation; from which they are again displaced by warmer particles. Thus, the temperatures reversing the situations, and the situations reversing the temperatures, a circulation is kept up, tending to restore the equilibrium.

Precisely similar would be the case with our atmosphere, were it not an elastic fluid, and dependent for its density on pressure as well as on heat. Its temperature would be much more uniform than at present—and all its variations would be gradual. An interchange of position would incessantly take place, between the colder air of the upper regions, and the warmer, and of course lighter, air, near the earth's surface, where there is the most copious evolution of solar heat. Currents would incessantly set from the poles to the equator below, and from the equator to the poles above. Such currents would constitute our only winds, unless where mountains might produce some deviations. Violent gales, squalls, or tornadoes, would never ensue; gentler movements would anticipate them. But the actual character of the air, with respect to elasticity, is the opposite of that which we have supposed. It is perfectly elastic. Its density is dependent on pressure, as well as on heat; and it does not follow, that air which may be heated, in consequence of its proximity to the earth, will give place to colder air from above. The pressure of the atmosphere varying with the elevation, one stratum of air may be as much rarer by the diminution of pressure, consequent to its altitude, as denser by the cold, consequent to its remoteness from the earth—and another may be as much denser by the increased pressure arising from its proximity to the earth, as rarer, by being warmer. Hence when unequally heated, different strata of the atmosphere do not always disturb each other. Yet after a time, the rarefaction in the lower stratum, by greater heat, may so far exceed that in an upper stratum, attendant on an inferior degree of pressure, that this stratum may preponderate, and begin to descend. Whenever such a movement commences, it must proceed with increasing velocity; for the pressure on the upper stratum, and of course its density and weight, increases as it falls; while the density and weight of the lower stratum must lessen as it rises. Hence the change is at times so much accelerated, as to assume the characteristics of a tornado, squall, or hurricane. In like manner may we suppose the predominant gales of our climate to originate. Dr. Franklin long ago noticed, that north-eastern gales are felt in the south-westernmost portions of the continent first; the time of their commencement being found later, as the place of observation is more to the windward.

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[*] From the Journal of the Academy of Natural Sciences of Philadelphia.

The Gulf of Mexico is an immense body of water—warm, in the first place, by its latitude,—in the second place, by its being a receptacle of the current produced by the trade-winds, which blow in such a direction as to propel the warm water of the torrid zone into it, causing it to overflow and produce the celebrated Gulf stream, by the ejection, to the north-east, of the excess received from the south-east. This stream runs away to the northward and eastward of the United States, producing an unnatural warmth in the ocean, as well as an impetus, which, according to Humboldt, is not expended until the current reaches the shores of Africa, and even mixes with the parent flood under the equator. The heat of the Gulf stream enables mariners to ascertain by the thermometer when they have entered it: and in winter, this heat, by increasing the solvent power of the adjoining air, loads it with moisture—which, on a subsequent reduction of temperature, is precipitated in those well-known fogs with which the north-eastern portion of our continent, and the neighbouring seas and islands, especially Newfoundland and its banks, are so much infested. An accumulation of warm water in the Gulf of Mexico, adequate thus to influence the ocean at the distance of two thousand miles, may be expected, in its vicinity, to have effects proportionally powerful. The air immediately over the Gulf must be heated, and surcharged with aqueous particles. Thus it will become comparatively light: first, because it is comparatively warm; and in the next place, because aqueous vapour, being much lighter than the atmospheric air, renders it more buoyant by its admixture.

Yet the density, arising from inferiority of situation in the stratum of air immediately over the Gulf, compared with that of the volumes of this fluid lying upon the mountainous country beyond it, may to a certain extent more than compensate for the influence of the heat and moisture derived from the Gulf: but violent winds must arise, as soon as these causes predominate over atmospheric pressure, sufficiently to render the cold air of the mountains heavier.

When, instead of the air covering a small portion of the mountainous or table land in Spanish America, that of the whole north-eastern portion of the North American continent is excited into motion, the effects cannot but be equally powerful, and much more permanent. The air of the adjoining country, first precipitates itself upon the surface of the Gulf, and afterwards, that from regions more distant. Thus a current from the north-eastward is produced below. In the interim, the air displaced by this current rises, and being confined by the table land of Spanish America, and in part, possibly, by the trade-winds, from passing off in any southernly course, it is, of necessity, forced to proceed over our part of the continent, forming a south-western current above us. At the same time its capacity for heat being enlarged, by the rarefaction arising from its increased altitude, much of its moisture will be precipitated; and the lower stratum of the south-western current, mixing with the upper stratum of the cold north-eastern current below, there must be a prodigious condensation of aqueous vapour.

The reason is obvious why this change is productive only of north-eastern gales—and that we have not northern gales, accompanied by the same phænomena. The course of our mountains is from the north-east to the south-west. Thus no channel is afforded for the air proceeding to the Gulf, in any other course, than that north-eastern route which it actually pursues.

That the [illegible] lands of Mexico are competent to prevent the escape over them of the moist warm air displaced from the surface of the Gulf, must be evident from the peculiar dryness of their climate—and the testimony of Humboldt. According to this celebrated traveller, the clouds formed over the Gulf never rise to a greater height than four thousand nine hundred feet; while the table land, for many hundred leagues, lies between the elevation of seven and nine thousand feet. Consistently with the chemical laws which have been experimentally ascertained to operate throughout nature, air, which has been in contact with water, can neither be cooled nor rarefied, without being rendered cloudy by the precipitation of aqueous particles. It follows, that the air displaced suddenly from the surface of the Gulf of Mexico, by the influx of cold air from the north-east, never rises higher than the elevation mentioned by Humboldt as infested by clouds. Of course it never crosses the table land, which, at the lowest, is 2000 feet higher.

Our north-western winds are produced, no doubt, by the accumulation of warm moist air upon the surface of the ocean, as those from the north-east are by its accumulation on the Gulf of Mexico. But in the case of the Atlantic, there are no mountains to roll back upon our hemisphere the air displaced by the gales which proceed from it, and to impede the impulse, thus received, from reaching the eastern continent. Our own mountains may procrastinate the flood, and consequently render it more lasting and violent, when it can no longer be restrained. The direction of the wind is naturally at right angles to the boundary of the aquatic region producing it, and to the mountainous barrier which delays the crisis.

The course of the North American coast is, like that of its mountains, from north-east to south-west; and the gales in question are always nearly north-west, or at right angles to the mountains and the coast. The dryness of our north-west wind may be ascribed not only to its coming from the frozen zone, where cold deprives the air of moisture, but likewise to the circumstance above suggested, that the air of the ocean is not, like that of the Gulf, forced back over our heads to deluge us with rain.

Other important applications may be made of [illegible] chemical knowledge. Thus, in the immense capacity of water for heat, especially when vaporized, we see a great magazine of nature provided for mitigating the severity of the winter.—To cool this fluid, a much greater quantity of matter must sustain a proportionable increase of its sensible heat.—Aqueous vapour is incessantly a vehicle for conveying the caloric of warmer climates to colder ones. Mistaking the [illegible] cause, snow is considered as producing cold, by the ignorant; but it has been proved, that as much heat is given out during the condensation of aqueous vapour as would raise twice its weight of glass to a red heat. Water, in condensing from the aëriform state, will raise ten times its weight one hundred degrees. The quantum of caloric which can raise ten parts one hundred degrees, would raise one part one thousand degrees nearly (or to a red heat visible in the day); and this is independent of the caloric of fluidity, which would increase the result.

Further,

Further, The quantum of heat which would raise water to 1000, would elevate an equal bulk of glass to 2000. Hence we may infer, that from every snow there is received twice as much caloric as would be yielded by an equal depth of red-hot powdered glass.

It is thus that the turbulent wave, which at one moment rocks the mariner's sea-boat on the border of the torrid zone, transformed into a cloud, and borne away towards the arctic, soon after supports the sledge or the snow-shoe of an Esquimaux or Greenlander; successively cooling or warming the surrounding media, by absorbing or giving out the material cause of heat.

XIX. *On the Number and Situation of the Magnetic Poles of the Earth. By Professor* CHRISTOPHER HANSTEEN *.

THE attraction of iron by the magnet was known to the naturalists of Greece and Rome, but it is uncertain at what time the Europeans became acquainted with that remarkable property of the magnet which we call Polarity; distinct traces, however, of the use of the compass are found towards the end of the twelfth century. There is no doubt that the Chinese knew it long before, and it is very probable that the Venetians obtained some information respecting it while trading on the Red Sea.

Our Northern ancestors were in this respect not behind the inhabitants of Southern Europe, as may be seen in the *Landnamabok*, part i. chap. 2 and 7, where we are told that the famous Viking Floke Vilgerdarson, the third discoverer of Iceland, who sailed about the year 868 from Rogaland in Norway, in order to seek for *Gardarsholm* (Iceland), took three ravens with him, which were to serve him as guides. For on letting birds fly on the open sea, and finding them to return, it was considered as a sign of there being no land near. But if they flew away, the vessel followed them, with a view, of reaching the nearest shore. In order to consecrate these ravens for this purpose, Floke offered up a great sacrifice at *Smörsund*, where the ships lay ready for sailing; for "at that time the navigators in the Northern countries had no magnets" (*þviat þa höfdo hafsiglingarmen enger* leidarstein *i þan þima á nordorlöndum*). As the *Landnamabok* was apparently written at the close of the eleventh century, the polarity of the magnet must then have been known in the North, although the passage just quoted does not imply the actual existence of a regular compass*.

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* From Dr. Kæmtz's translation into German of the original memoir, published in the *Magazin for Naturvidenskaberne, udgivet af Professorer ne Lundh, Hansteen og Maschmann*, vol. i. p. 1—46.

The circumstance of a freely moveable magnet turning constantly with its poles to the north or south, leads us to the conclusion that the earth itself must be a large magnet, which has near the geographical arctic pole a pole like that of the magnetic needle turned towards the south, and near the antarctic geographical pole a magnetic pole like that of the magnetic needle turned towards the north. If the magnetic needle were, in every part of the earth, to point due north and south, we might say without hesitation that the magnetic poles corresponded with the geographical. However, after the compass had been used for several centuries, it was found, on closer investigation, that the magnetic needle actually deviates from the meridian:

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* This work was published at Copenhagen in 1774, under the title of Islands Landnamabok. *Hoc est;* "Liber originum Islandiæ. Versione Latina, lectionibus variantibus et rerum, personarum, locorum, nec non vocum rarissimarum indicibus illustratus. Ex manuscriptis Legati Magnæani. 4." The editor of this book names himself at the end of the preface Johannes Finnæus. This work, in which the position and condition of Iceland, as well as the history of its industrious inhabitants, is given at large, had several authors. The first of them was (*Landnamabok*, p. 378), Arius Polyhistor (*Ari prestrhinn Frodi Thorgilssun*), born in the year 1068; and the last, Hauk, son of Erlend (*Haukr Erlendssun*), who died in 1334. For in the Latin version (Lib. v. cap. 15. p. 378) it is said: "Hunc autem librum Dominus Haukus Erlendi filius secundum librum, quem Dominus Sturla filius Thordi Nomophylax vir eruditissimus concinnaverat, et secundum alium librum, a Styrmere Polyhistore exaratum, scripsit, et ex quovis libro ea quæ uberius enarrata erant, retinuit, maxima autem ex parte uterque liber eadem referebant; non igitur mirum hunc Landnamabok omnibus aliis prolixiorem esse." The passage quoted by Professor Hansteen appears indeed in the beginning of the work, and it might thence be inferred that it was written by Arius: yet this is not certain, and it might easily have been added by later editors. Moreover the editor says in reference to this passage (p. 7), "Hoc caput," the second in which the passage appears, "est secundum Hauksbok," as he calls it according to its author Hauk; and, what is more, the passage (according to the editor) is missing in three different manuscripts. It is, therefore, yet to be doubt[illegible] passage be genuine, and whether the Icelanders knew the magnet at so early a period. That they knew the deviation of the needle as early as 1260, appears from a manuscript of Adsigerius in the library at Leyde[illegible] which Professor Hansteen (Investigations respecting the Magnetism of the Earth, p. 403) seems to have known only from Thevenot's account. The words, according to Van Swinden (*Bibliothèque Universelle*, tom. xxiv. p. 262), are as follows: "Nota quod partem meridionalem acus, in usu directorii debemus facere declinare per unum punctum versus occidens, et hoc debet fieri per declinationem partis septentrionalis ad oriens, quia pars meridiana instrumenti divisionibus caret. Nota quod lapis magnes, ut ut exactius consectatus tamen non directe tendit ad polos, sed pars, quæ ad meridiem tendere

further, that the deviation is different in various places, being in some more westerly and others more easterly: at last, too, it was found that the deviation differed at different times in the same places. These phænomena can only be explained by the assumption that the magnetic poles do not correspond with the geographical, and change their position from year to year. As there are, however, natural magnets having four poles, two and two of the same denomination, the earth itself may possibly be such an anomalous magnet. Thus, then, the two following questions are to be answered: *Are two magnetic poles sufficient to explain all the phænomena of the declination, or must we assume several of them? What is the position and motion of these poles?*

Our curiosity for obtaining a better knowledge of the magnetic state of the earth must be excited from its importance to navigation; but it is increased by the prospect of the light that thereby may be thrown on natural philosophy. It is impossible

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tendere reputatur, aliquantum declinat ad occidens, illa quæ ad septentrionem respicere creditur, tantumdem ad oriens se inclinat. Quanta autem sit hæc inclinatio, inveni multis experimentis versus 5 gradus." .... The work is dated "in castris et obsidione..... (name illegible) anno Domini 1269, 8° die Augusti."

I will take this opportunity of adding another historical observation. Both Hansteen (Magnetism of the Earth, p. 405) ,and after him Horner (Gehler's Physical Lexicon, n. ed. vol. i. p. 137), believe, that Father Gay Tachart first discovered in the year 1682 that the advance of the needle was not regular, but was subject to variations. The same subject is, however, already mentioned in the Philosophical Transactions, vol. iii. no. 37, p. 726, in an Extract of a Letter written by Dr. B. to the publisher, concerning the present Declination of the Magnetic Needle, dated 23rd of May 1668. The author of this letter says that he had received these observations from Capt. Samuel Sturms, an experienced seaman, who had made them in presence of the mathematician Staynred, near Bristol, on the 13th June 1666. These observations are as follows:

Height of the Sun.	Azimuth of the Sun with the Magn. Merid.	True Azimuth of the Sun.	Declination.
° ′	° ′	° ′	° ′
44 20	72 0	70 38	1 22 W.
39 30	80 0	78 24	1 36
31 50	90 0	88 26	1 34
27 42	95 0	93 36	1 24
23 20	103 0	101 23	1 23

These observations were repeated by this gentleman, in the same place, on the 13th June 1667, and he then found that the declination was 6′ more west. The same seaman also said, that in different places he found a difference in the declination from 2′ to 7′.—Kæmtz.

for us to dive with our bodily eyes into the bowels of the earth, the greatest depth to which we have arrived being trifling compared with the actual diameter of the globe:—yet the interior of the earth is revealed by its effects on its surface. Thus, the experiments on the deviation of the plummet from the vertical line in the vicinity of mountains, show that the mean density of the mass of the earth is five times greater than that of water; and that consequently this mass is denser than most kinds of stone, and is therefore for the most part metallic. Thus the periodical annual and diurnal motions of the magnetic needle are a mute language, telling us what is passing within the earth. Thus the *aurora borealis* is perhaps the result of a contest of powers set in motion by the different substances of the earth, substances which by these means may one day become known to us. For we may justly conclude of the causes from their effects, which is the usual way of extending our knowledge of nature.

Yet, although this investigation affords a great interest both for theory and practice, it is not every person's business to enter into mathematical investigations. I thought, therefore, that I might gratify many readers in giving here a popular sketch of the results of my investigations on terrestrial magnetism.

The accompanying charts (Plates I. and II.) represent two segments of the surface of the earth from the poles to the 50th degree of latitude. The longitudes are calculated from the meridian of Greenwich, as most observations have been made by British seamen, who calculate from that meridian. The arrows on the charts indicate the directions of the magnetic needle; the end of them, towards the opposite side of the pole, denotes the place of observation; and the angle formed by the geographical meridian with this end of the arrow is therefore the variation of the needle found in the observation. The observations given on the southern chart are all Captain Cook's, and were made between the years 1772 and 1777: the observations on the northern segment are by Captains Cook and Phipps, Admiral Lövenörn, Captain Billings, and others, made about the same period. Some of them have the time of the observation affixed to them. The most important observations made during the last English north-polar expedition (1818—1820) are marked with an asterisk. As these observations embrace so short a space of time, they may be considered as being contemporaneous, and thus show the magnetic condition of the earth in the vicinity of the poles during the quarter of a century just elapsed.

The variation in all Europe is now westerly. If we go from east to west by the Atlantic Ocean to Greenland, it increases in proportion as we approach the southernmost points of that country. Thus it is at St. Petersburg about = 8° W., at Stockholm = $15\frac{1}{2}$°, in Christiania = 20°, in London = $24\frac{1}{3}$°, on the north coast of Iceland above 40°, and in the colony of Godthaab in Greenland above 51°. From the western coast of Greenland to Hudson's Bay it decreases again by some degrees: but in Hudson's Bay this decrease is so strong, that in the year 1769 it was found in fort Prince of Wales, on the western coast of the bay, to have been only = 9° 41′. If we proceed on the continent, it disappears entirely, becomes then easterly, and increases so rapidly towards the western coast of America, that it was, according to the observation of Cook in the year 1778, in Nootka Sound = 19° 51′ E.; and in the same year, in the northernmost part of Behring's Straits, = 35° 37′ E. If we extend the arrows in Nootka Sound and in Hudson's Bay and Strait, we see them meet in one point, which is about 20° distant from the pole, and about 259° east of Greenwich.

In fact, every apparently straight line on the surface of the earth is the arc of a great circle. If, then, we wish to determine the situation of this point more accurately, we must combine some of the above-mentioned points of declination, two by two, (for instance, in Nootka Sound and in fort Prince of Wales), and calculating, according to the rules of spherical trigonometry, the situation of the point of meeting of these prolonged lines of magnetical direction (the magnetical point of convergence), we shall obtain as many determinations of the same as we have pairs of observations. In order to obtain the situation of this point, I have made use of the following observations:

Observer.	Place of Observation.	Time.	North Lat.	West Long. from Lond.	Declin. West.	Nos.
Hutchins	Hudson's Strait	1774. July 23	62° 3′	69° 0′	43° 0′	1
		— 27	62 23	71 30	42 50	2
		— 28	62 25	71 30	44 0	3
	Hudson's Bay	Aug. 14	56 53	85 22	28 0	4
	Fort Moose	Sept. 8	51 20	82 30	17 0	5
	Fort Albany	— 14	52 22	82 30	17 0	6
Wales	Ft. Pr. of Wales	1769. — —	58 47	94 4	9 41	7

Among these observations the 7th can be most relied upon, having been made on shore by the astronomer Wales, by m[illegible] of a large compass and an exact meridian, and being the mean of 21 observations made within a few days. If we calculate from them the situation of the point of convergence, the following will be the result:

From Nos.	Situation of the Point of Convergence.	
	Distance from the Pole.	Longitude West from London.
	° ′	° ′
2 and 7	19 44	99 53
1 — 7	19 42	99 54
1 — 5	19 32	101 24
3 — 4	19 23	105 20
Mean	19 33	101 45

As the result of the 4th observation deviates considerably from the others, and having moreover reason to believe that I have noted it wrong, we will omit it entirely*. As, moreover, from the reasons given above, we must consider the 7th observation the most accurate, I thought that I ought only to rely on the mean obtained from the determinations 2—7 and 1—7, by which we obtain the following for the determination of the point of convergence in 1769:

Distance from the pole . . . . . . = 19° 43′
Longitude W. of London . . . . = 99 53½
——— E. of Greenwich . . . = 259 58

If in the North we follow the coast of Norway, the declination decreases, and at last entirely disappears in the White Sea. Thus Bohr found it in the year 1818, in Bergen = 24° 18′ W.; Lieut. Christie, in Vadsöe in Varangerfiorden, the 28th of June of the same year, = 7° 55′. In the year 1770, in the vicinity of Spitzbergen, Captain Phipps found the declination in some places between 11° and 12°, and in others about 20°. Capt. Buchan and Lieut. Franklin found the declination in the discovery ships Dorothea and Trent, in the year 1818, in most places near Spitzbergen about 24°. If we draw on these places in the chart arrows forming the above-mentioned angles with the meridian, their continuation will not go through the point which we found above at 19° 43′ from the pole, and 259° 58′ E. of Greenwich. The same will be the case with the extension of the arrows that may be drawn in the northernmost parts of Behring's Straits and in north-eastern Siberia. By this we are led to the supposition that there must be somewhere in the Siberian Ocean a magnetic pole which attracts the northern pole of the needle,—in the sea between Spitzbergen and Norway, towards the east, and in eastern Siberia and Behring's Straits, towards the west.

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* "The observation No. 4. is given by Lambert (*Astron. Jahrb.* 1779, p. 148) as follows: Variation 24° 0′ W. Long. 292° 11′. Lat. 56° 33′. The point of convergence is calculated after this. On the other hand, you will find in my extracts from the Philosophical Transactions for 1775, the latitude = 56° 53′, and the declination = 28° 0′ W., which would remove the result still further. As I have not the above work at hand, I cannot make out where the error lies."—On the Magnetism of the Earth, p. 90, note. According to the Philosophical Transactions for 1775, p. 135, Lamber[illegible] correct: Hutchins, however, adds: "These experiments were made in conjunction with Capt. Richards, in the cabin of the Prince Rupert, whilst she lay among the ice. The ship frequently varied the position of her head a point of the compass; but by replacing the instrument as often as we found [illegible]sion, I have the greatest reason to think these observations (which t[illegible] nearly three hours) are pretty accurate." Under all these circumst[illegible] we may be justified in entirely overlooking [illegible] observation.—K.

The following observations may serve to determine the position of this point of attraction *.

Place of Observation.	Year.	N. Lat.		Long. from Ferro.		Variation.			Nos.
		°	′	°	′	°	′		
Casan	1761	55	48	67	1	2	25	W.	1
	1805	—	—	—	—	2	2	E.	2
Katharinenburg	1761	56	51	78	2	0	50	E.	3
	1805	—	—	—	—	5	27	E.	4
Tobolsk	1761	58	12	85	46	3	46	E.	5
	1805	—	—	—	—	7	9	E.	6
Jakutskoi	1768	62	2	147	21	5	15	W.	7
	1769	—	—	—	—	5	0	W.	8
	1788	—	—	—	—	2	0	W.	9
Ustkameno-gorskaio	1770	49	56	100	20	2	0	E.	10
Barnaul	1770	53	20	101	11	2	45	E.	11
Perm	1805	58	1	74	6	1	10	E.	12
Tara		56	55	91	45	6	6	E.	13
Tomsk		56	30	107	50	5	37	E.	14
Nizni Udinsk . . .		54	55	116	42	2	40	E.	15
Irkutsk		52	17	121	51	0	32	E.	16

Thence we see that the western declination entirely disappeared in 1805, before we arrive at Casan; from Casan to

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* The observations for the year 1805 are by the counsellor of state Schubert, and are found in Bode's *Astron. Jahrb.* 1809: the others are by different literati who resided in various parts of Siberia in order to observe the transit of Venus through the Sun in the years 1761 and 1769, and are given in Bode's *Jahrbuche* for 1779.—H.

Tobolsk the *eastern* declination increased; and again decreased towards Irkutsk, where it was only = ½°. Further east it must vanish entirely; for in Jakutskoi, Billings found in 1788, a westerly declination of 2°. Further east from Jakutskoi this western declination disappears again, and becomes in Kamtschatka, and the whole of north-western America, again *easterly*. Thus we see that there are round the north pole four places where no declination is found: viz. 1st, on the west coast of Hudson's Bay; 2dly, in the line between the White Sea and Casan; 3rdly, a little eastward of Irkutsk; and 4thly, a little eastward of Jakutsk. Between the first and second distance, *i. e.* in north-eastern America, the Atlantic Ocean, and all Europe, the declination is westerly; between the 2d and 3rd, *i. e.* in the greater part of Siberia, it is easterly; between the 3rd and 4th, *i. e.* in eastern Siberia, it is westerly; and between the 4th and 1st, *i. e.* in Kamtschatka, the northern part of the Pacific Ocean, and the north-west part of America, it is again easterly.

If we continue the arrows which point out the direction of the magnetic needle in Siberia in the year 1805; for instance, in Tobolsk, Tara and Udinsk, we see them converge in one point, situated about 5° from the pole, and between the meridians 110° and 120° E. of Greenwich. If we combine the observations, by pairs, and thereby calculate the position of the magnetic point of convergence, we have the following results:

From Nos.	Distance from the Pole.		Longit. from Ferro.	
	°	′	°	′
13 and 15	4	27	134	7
6 — 15	4	50	133	31
6 — 14	3	51	155	54
6 — 16	5	16	124	58
Mean . .	4	36	137	7½

Thus the mean of all gives the distance from the pole at 4° 36′, and the longitude from Ferro =137° 7′½ : but a mean of the two first which agree best, gives the distance from the pole at = 4° 38′ 30″, and the longitude from Ferro = 133° 49′, or 116° 9′ from Greenwich.

From the above observations it appears that the declination in Siberia has changed every where from 1761 to 1805. Thus at Casan, it was in the year 1761 = 2° 25′ W., in the year 1805 = 2° 2′ 30″ E., or in forty-four years it had a change of = 4° 27′ 30″, or 6′·1 per annum. The change in Catharinenburg during the same period is = 4° 37', or 6'·3 per annum; in Tobolsk = 3° 23', or = 4'·6 per annum; in Jakutskoi, from 1768 to 1788, = 3° 15', or = 9'·7 per ann. Thence we find by interpolation, that in 1770 the declination in Jakutskoi was = 4° 50' W., in Tobolsk = 4° 27' E., and at Barnaul 2° 45'. If we pair these declinations in the usual manner, we find the situation of the point in 1770:

According to the Observations	Distance from the Poles.	Longit. from Ferro.
	° '	° ' ''
In Tobolsk and Jakutskoi	4 4	117 31 0
In Barnaul and Jakutskoi	4 24	120 48 0
Mean . . .	4 14*	119° 9' 30''

If we compare with this the above result for the year 1805, we find the distance from the pole to have remained nearly the same, but that the longitude of this point increased from 1770 to 1805; the change during these 35 years having been = 133° 49' − 119° 9' 30'' = 14° 39' 30'', or 25'·128 per annum. *Thus this magnetic pole has a motion from west to east.* Whether its course be a circle round the terrestrial pole, or a differently curved line, or whether it be merely an oscillation, must be learned from the experience of future ages. If we assume a uniformly circular motion, the period of the revolution, according to the degree of velocity found above, would be 860 years.

Whether the magnetic point of convergence found above in North America be also moveable, must be determined by calculating its position from older observations, and comparing it with that of the year 1769.

The following observations of declinations, made at the Fort Prince of Wales, distinctly show that this point has a perceptible motion towards the east:

		annual change.
By Chr. Middleton in	1725=21° 0' W.	
... ...	1738=18 0	13'9
... ...	1742=17 0	15 0
By W. Wales ...	1769= 9 41	16 3
	1798= 1 0 E.	22 1 }†
	1813= 6 0 E.	20 0 }†

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* In the original, as well as in the work on the magnetism of the earth, p. 94, it is stated by mistake at 4° 17' instead of 4° 14'.

† These two observations are from the MS. journal, entitled His Majesty's sloop

From these observations it would appear that the declination in Fort Prince of Wales in the year 1795, was =0; that therefore the magnetic converging point lay north of it, viz. in the meridian 265° 48′. We have seen above that in the year 1769 it lay in 209° 58′; and consequently that from 1769 till 1795, *i.e.* in the space of 26 years, this point moved 5° 50′ from west to east, by which the annual variation would amount to 13′·45. The following observations made in Hudson's Bay in 1813, and which are also extracted from the above-quoted log-book, will determine the point more exactly.

1813.	North Lat.	Long. W. from Greenwich.	Declination.	Nos.
	° ′	° ′	° ′	
Aug. 1	62 16	70 17	50 0 W.	1
11	62 47	80 17	45 0	2
Sept. 3	58 48	94 16	6 0 E.	3
23	58 18	88 50	10 0 W.	4
25	60 35	81 30	36 0 W.	5

Calculating these observations by pairs, in the usual manner, we find the following situation of the American point of convergence:

From Nos.	Distance from the Poles.	Long. W. from Greenwich.
	° ′	° ′
1 and 3	21 44	91 35
2 — 3	23 40	92 18
1 — 4	22 9	93 22
3 — 5	23 47	92 21
Mean	22 50	92 24

According to some older observations by Chr. Middleton, I have laid down the situation of this point for the year 1730, in my work on the magnetism of the earth, (p. 90, 91,) as follows. Distance from the pole = 19° 43′, and eastern longitude

sloop Brazen's Remark-book between the 31st of June and 24th of November 1813, in Hudson's Bay; which I read in the year 1819, together with a great many other ship-journals and log-books in the Marine Chart Office of the Admiralty in London.—H.

from Greenwich = 108° 6'. If now we place these three determinations together, we obtain:

	Distance from the Pole.	Long. W. from Greenwich.
1730	19°15'	108° 6'
1769	19 43	100 2
1813	22 50	92 24

Which distinctly shows that this magnetic pole has also a perceptible motion towards the east; and it seems also to follow that it moves away from the terrestrial pole. From the year 1730 to 1796, *i.e.* within 39 years, it has moved 8° 4', or 12'·41 in every year more east; from 1769 to 1813, *i.e.* within 44 years, this motion amounted to 7° 38', or 10'·41 annually. Whether this difference arises from an inequality in the motion or an error in the observation, we must leave to the decision of future generations.

As the northern pole of the magnetic needle is directed towards this point in the whole of North America, we seem to be justified in concluding, that if we were to travel round it with a compass, the needle would in that time make a complete revolution. If then we are south of this point, the northern pole of the needle will point due north, or in other words, there will be no variation at all on this spot: to the north of it the northern pole would point to the south, or the declination would be 180°; to the east of it the declination would be 90° W., and to the west it would be 90° E. The justness of this conclusion is proved from the observations of Captains Ross and Parry in the years 1818, 1819 and 1820, some of which are marked on the chart. Most of these arrows, as may be seen, are directed to one point; and the situation of it in the year 1820 might be determined in the manner described above. As these observations are very important for the theory, and we may probably have no speedy opportunity of making observations in these inaccessible parts, I shall proceed to give the most remarkable of them.

[To be continued.]

XX. *On the Combinations of Antimony with Chlorine and Sulphur. By M.* Henri Rose*.

I. *Combinations of Antimony and Chlorine.*

WHEN pulverized antimony is distilled with an excess of corrosive sublimate, it is known that there is obtained a solid compound of antimony and chlorine, which melts at a very moderate heat. It attracts the humidity of the air, and is converted into a liquid similar to an emulsion*.

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* From the *Annales de Chimie*, tom. xxix.

Treated with water it changes, without giving out heat, into hydrochloric acid and a compound of the oxide and chloride of antimony. This white powder, which is precipitated by mixing the chloride with water, is entirely volatilized when heated by a blowpipe in a little matrass; it contains therefore, neither antimonious acid nor antimonic acid: but as this chloride of antimony is converted by water into hydrochloric acid and oxide of antimony, it must correspond to them in composition; and as oxide of antimony contains 3 atoms of oxygen, the antimony must be combined with 3 atoms of chlorine in the solid chloride of antimony, or contain

Antimony	54·85
Chlorine	45·15
	100·00

Yet as Dr. John Davy's analysis of this solid chloride of antimony gives a different result†, I analysed it in the following manner. I poured water on a quantity of the chloride, and added tartaric acid, until the liquid was perfectly clear and ceased to become milky by adding afresh a large quantity of water. I then passed a current of sulphuretted hydrogen through the liquid, till sulphuret of antimony was no longer precipitated. This sulphuret, which was orange-coloured, was washed on the filtre, weighed and dried, then melted in a glass tube; it gave a black sulphuret of antimony, and only traces of sulphur: it was therefore the sulphuret of antimony containing 3 atoms of sulphur, or precisely what ought to be formed under these circumstances. As, however, it contained traces of an excess of sulphur, in consequence of the sulphuretted hydrogen which had been passed for a very long time through the liquid, I heated a part of this sulphuret in a bulb blown in the middle of a glass tube, and passed over it a current of hydrogen dried by chloride of calcium. The sulphuret of antimony was decomposed; and there was obtained antimony, sulphuretted hydrogen, and traces of sulphur.

The liquor, separated from the sulphuret of antimony, was slowly heated, to drive off the sulphuretted hydrogen, but not

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* The ordinary butter of antimony in pharmacy, which forms a clear liquid, is not a solution of the solid chloride of antimony in a small quantity of water, but in muriatic acid; for the Pharmacopœias prescribe for its preparation a greater quantity of acid than is necessary for the formation of the solid chloride.

† According to Dr. Davy, the chloride contains:

Antimony	60·42
Chlorine	39·58
	100·00

the hydrochloric acid, which cannot be separated from water by heat when it is mixed with it in small proportion. The hydrochloric acid was then precipitated by nitrate of silver. The chloride of silver obtained had notwithstanding a blackish colour, from a slight mixture of sulphuret of silver. The results of this analysis were: Antimony 1·937 gramme (29·9 grs.), and chloride of silver 6·886 grammes (106·3 grs.), equivalent to 1·699 gramme (24·7 grs.) of chlorine. The chloride of antimony then is composed of

Antimony	53·27
Chlorine	46·73
	100·00

If I had obtained the chloride of silver quite free from sulphuret of silver, this result would agree much more with the calculation.

If a current of dry chlorine is made to pass over heated metallic antimony, another chloride of antimony is obtained. The antimony burns vividly in the gas, emitting sparks, whilst a very volatile liquid is formed. This liquid is white, or of a very light yellowish tint; it also contains chloride of iron, if the antimony employed contained a portion of this metal. The chloride nevertheless remains at the bottom of the vessel, and does not dissolve in the liquid. This resembles, in all its external characters, the fuming spirit of Libavius; it has a strong and disagreeable smell, and fumes in the atmosphere. When exposed to the air, it attracts water and changes into a white mass, in which white crystals form, which afterwards dissolve without rendering the solution milky. This phænomenon is caused by a property of the liquid chloride of antimony, (which it possesses in common with the fuming spirit of Labavius,) of forming a crystalline mass when mixed with a little water.

The liquid chloride of antimony heats strongly when mixed with a greater quantity of water; it becomes milky, and a precipitate is formed having the properties of hydrated antimonic acid. Heated gently, it gives off water and becomes yellowish; but at an elevated temperature it becomes white. The liquid contains hydrochloric acid. As the liquid chloride of antimony is changed by water into the hydrochloric and antimonic acids, which last contains 5 atoms of oxygen to 1 of antimony, it follows that this chloride contains 5 atoms of chlorine to 1 of antimony, or

Antimony	42·15
Chlorine	57·85
	100·00

I analysed the liquid chloride of antimony exactly in the same manner as the solid chloride. By sulphuretted hydrogen I obtained sulphuret of antimony; also orange-coloured, but a little paler than the sulphuret obtained in analysing the solid chloride. It contained 5 atoms of sulphur to 1 of antimony. Treated with dry hydrogen, it is converted into metallic antimony and sulphur, and sulphuretted hydrogen is disengaged. I obtained 1·980 grammes (30·6 grs.) of metallic antimony; and the liquid, separated from the sulphuret and precipitated by nitrate of silver, gave 11·764 grammes (181·6 grs.) of chloride of silver, equivalent to 2·902 grammes (44·8 grs.) of chlorine. The chloride of silver, however, contained a little more sulphuret of silver than that obtained in the analysis of the solid chloride. The result of this analysis is, then, 40·56 of antimony, and 59·44 of chlorine; which differs from the calculated result: but the difference is produced solely by the sulphuret of silver which is left mixed with the chloride.

It is not the liquid chloride of antimony that is obtained when dry chlorine is passed over sulphuret of antimony containing 3 atoms of sulphur, but it is the solid chloride of antimony and the chloride of sulphur which are formed. The chloride of sulphur may be separated from the chloride of antimony by gently heating them in a very narrow-mouthed matrass: there remains then only chloride of antimony. This is the same product which is formed when gray copper is analysed by chlorine; chloride of antimony containing 3 atoms of chlorine, and chloride of sulphur containing 2 atoms of chlorine only are obtained. There is no double chloride formed, the chloride of sulphur remains on the solid chloride of antimony. Heated gently, so as merely to fuse the chloride of antimony, the latter dissolves completely in the chloride of sulphur, and forms with it a homogeneous liquid; but the chloride of antimony is precipitated in crystals on cooling. This is one way of obtaining large crystals of this chloride; but it must be filtered quickly through blotting-paper, to separate them as much as possible from the adhering chloride of sulphur.

It is remarkable that the liquid chloride of antimony is produced only by the action of chlorine on metallic antimony, but that none is formed if the chlorine is made to act on the sulphuret of antimony.[*]

II. *Com-*

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[*] I several times passed chlorine over sulphuret of antimony, and always found the same result. I imagined, for reasons which I shall hereafter state, that chloride of antimony with 5 atoms of chlorine was formed. Yet I only obtained chloride with 3 atoms, if I drove off the chloride of sulphur. I was then induced to believe that 2 atoms of chlorine were separated from the chloride of antimony, and had combined with the chloride of sulphur; with

II. *Combinations of Antimony and Sulphur.*

I have made many experiments on the sulphurets of antimony, and only found three which correspond with the oxides of that metal.

The sulphuret of antimony with 3 atoms of sulphur has different colours. That which is found native is of a lead-gray. Its composition has been made known by Berzelius. It is analogous to the oxide of antimony, with 3 atoms of oxygen; for it dissolves without residuum in hydrochloric acid, disengaging only sulphuretted hydrogen.

The same sulphuret of antimony is obtained by passing a current of sulphuretted hydrogen through a solution containing oxide of antimony; but it is of an orange colour, nearly similar to that of the golden sulphuret. It becomes brownish by drying, and then takes an aspect more like kermes. This same sulphuret is obtained by passing sulphuretted hydrogen through a solution of tartar emetic, or through a solution of butter of antimony in water and tartaric acid.

The kermes mineral is, as M. Berzelius first showed, of a composition exactly similar. Its colour, however, is brownish red*.

The deuto-sulphuret of antimony with 4 atoms of sulphur is of an orange colour, very like that of the golden sulphuret. It is formed, if sulphuretted hydrogen is passed through a solution of antimonious acid. Nevertheless, tartaric acid must not be added to enable the liquid to be diluted with water, but hydrochloric acid only†. The best way to obtain a solution of antimonious acid, is to dissolve antimony in aqua regia, and to evaporate the solution to dryness. Then the antimonic acid which is formed is changed into antimonious acid by a red heat; this is fused with caustic potash, and the melted mass is treated with hydrochloric acid and water till a clear liquor is obtained. I precipitated this solution by sulphuretted

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with which they had perhaps formed a chloride with 4 atoms of chlorine. I therefore passed some chlorine over chloride of sulphur, and carefully purified it by distillation from the sulphur dissolved, in order to detect such a chloride of sulphur. The chloride of sulphur indeed took a little darker colour; but there was no other change, although I made the chlorine pass over it for a long time.

* I analysed a kermes that I had prepared by digesting black sulphuret of antimony with a solution of carbonate of potash. I dried it at a moderate temperature, until it contained no more hygroscopic moisture, and decomposed it by hydrogen. 0·719 gramme (11·1 grs.) of kermes gave me 0·520 gramme (8 grs.) of antimony: its composition then was 72·32 antimony and 27·68 sulphur.

† Very remarkable results are obtained if tartaric acid is added to antimonious acid.—I shall make it the subject of a separate memoir.

hydrogen: the sulphuret obtained, after being carefully dried, was decomposed by hydrogen. I obtained in one trial 1·305 gramme (20·1 grs.) of antimony from 1·973 gramme (30·5 grs.) of sulphuret, and in another 0·977 gramme (15·1 grs.) of antimony from 1·468 gramme (22·7 grs.) of sulphuret. It is then composed, according to the first trial, of

Antimony	66·14
Sulphur	33·86
	100·00

and, according to the other, of

Antimony	66·55
Sulphur	33·45
	100·00

The composition, when calculated, is

Antimony	66·72
Sulphur	33·28
	100·00

The sulphuret of antimony with 5 atoms of sulphur to 1 of metal, which corresponds to antimonic acid, and which, by calculation, contains 61·59 of antimony and 38·41 of sulphur, is realized in the *golden sulphuret* of the shops. The different methods of its preparation are well known. It is also obtained if a current of sulphuretted hydrogen be passed through solutions which contain antimonic acid: as, for example, that of the liquid chloride of antimony in water, to which tartaric acid has been added. The precipitate obtained is of an orange colour, paler than the precipitate from solutions of oxide of antimony, and does not change colour in drying.

I analysed the golden sulphuret in two ways: I dried it at a heat insufficient to decompose it, till it no longer lost weight. It had then lost all its hygroscopic moisture. I generally made the analysis by passing a current of dry hydrogen over the heated golden sulphuret. Sulphuretted hydrogen was formed, but never water: sulphur was sublimated, and metallic antimony remained. I also analysed it by aqua regia, to which I added tartaric acid. I separated the undissolved sulphur, and precipitated the sulphuric acid by muriate of barytes: this method, however, is slower than that with hydrogen. An exact result is not obtained by fusing the golden sulphuret in a small matrass to convert it into sulphuret of antimony with 3 atoms of sulphur, and calculating the composition of the former from the weight of the latter; not only because the sulphuret of antimony is not absolutely fixed, but also because some oxide of antimony is formed by the air in the matrass, which produces a *crocus antimonii* with the sulphur sublimated in its neck.

I do not give the results of the analyses that I made of this sulphuret of antimony at a *maximum*, because they differ very little from the calculated result.

III. *Combinations of the Sulphuret of Antimony with Oxide of Antimony.*

In the Pharmacopœias, as is generally known, the names of *crocus* and *nitrum antimonii* are given to the compounds in which sulphuret of antimony combined with oxide of antimony in various proportions. Kermes has also been taken for such a compound. M. Berzelius, however, has shown that it does not differ in its composition from the sulphuret of antimony with 3 atoms of sulphur, and the analysis of kermes above given confirms this.

There exists, however, a combination of sulphuret of antimony with the oxide in a definite proportion, and that is the native kermes of mineralogists (*rothspiesglanzerz*). The result of the analysis which I made differs a great deal from Klaproth's, from his having supposed that the whole quantity of the antimony was both oxidated and sulphuretted, and from his having determined the quantity of antimony only*. I analysed the *rothspiesglanzerz* from Braunsdorf, near Freiberg in Saxony, which M. Weiss obligingly gave me for this purpose. The analysis was made by hydrogen, in the same manner as those of the different sulphurets of antimony. I added, however, to the apparatus a weighed tube containing chloride of calcium, to absorb the water formed. I obtained in one experiment 0·676 gramme (10·4 grs.) of antimony, and 0·054 gramme (0·84 grs.) of water, from 0·908 gramme (14 grs.) of the mineral, or 74·45 per cent of antimony and 5·29 of oxygen; and in another, from 0·978 gramme (15·1 grs.) of the mineral, 0·740 gramme (11·4 grs.) of antimony, and 0·047 gramme (0·73 grs.) of water, or 75·66 per cent of antimony and 4·27 of oxygen. I then dissolved 0·340 gramme (5·24 grs.) of the mineral in aqua regia; I added to the solution tartaric acid, and precipitated by muriate of barytes. I obtained 0·517 gramme (8 grs.) of sulphate of barytes, equivalent to 20·47 per cent of sulphur.

If the mean be taken of the oxygen of the first two analyses, that

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* Beitrage, t. iii. p. 182. The composition of this mineral is, according to him,

Antimony	67·80
Oxygen	10·80
Sulphur	19·70
	98·30

that is, 4·78 per cent, and the quantity of antimony required to form the oxide be added to it, the remaining quantity of metal is sufficient (slight errors of observation being neglected) to form with the sulphur the sulphuret of antimony with 3 atoms of sulphur. It will moreover be found, that the quantity of the oxide of antimony is to the quantity of sulphuret as the weight of an atom of the first is to the weight of 2 atoms of the second; so that the native kermes consists of 1 atom of oxide of antimony and 2 atoms of sulphuret of antimony, or of

Sulphuret of antimony . . .	69·86
Oxide of antimony	30·14

The chemical formula is then $\dddot{S}b + 2\,Sb\,s^3$, which M. Berzelius had already assigned for the composition of the native kermes. This composition is remarkable, as it offers the only example of a native crystallized oxy-sulphuret.

XXI. *On Mr.* BURNS's *Communications respecting the Double Altitude Problem.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

I CERTAINLY did not intend to notice further the communications of your correspondent Mr. Burns; but I *must* request you to point out a most singular misquotation which he makes from my last letter. I stated that "I noted the mistake in his assumption in italics;" Mr. B. quotes the remark thus, "I noted the assumption in italics."

No person acquainted with what has been done on the double altitude problem, will expect any notice to be taken of Mr. B's third and fourth solutions, as there is nothing new either in the principles of the solution or the formulæ employed.

Your obedient servant,

Greenwich Hospital, Feb. 18, 1826. E. RIDDLE.

To the Editor of the Philosophical Magazine and Journal.

Sir,

HAVING, with a little surprise, noticed a paper in your very useful work, No. 329, entitled "A short Method of finding the Latitude at Sea by Double Altitudes and the Time between," by James Burns, B.A., I beg leave to say that, though it certainly is a kind of double altitude, which he has investigated, it is not the problem that generally goes under that name, and which is so very puzzling to navigators in general:

neither

neither has he, in that paper, given a solution to the problem which he professes to solve.

The observations of Messrs. Riddle and Henderson are well founded; and I wonder you have not heard from more of your correspondents on the same subject: for after all he *has* advanced or *may* advance in its favour, it is evident he has proposed one problem and solved another. I do not hesitate a moment in saying, in the words of Mr. Riddle, that in his first paper "he has altogether misapprehended the nature of the problem." And though he has been practising it these six months, we have not yet received from him a *direct analytical solution* of a double altitude. The one he has given us at page 50, vol. lxvii., which is identical to the one at page 345, vol. lxvi., is the same in substance as those given in Kelly's Spheroids, Bonnycastle's Trigonometry, &c. &c.—as it represents no more than the several trigonometrical operations in algebraical terms.

The horary angles cannot be determined by any less laborious an investigation than the latitude itself; neither do the "Horary-Tables" show those horary angles at all. They only show the horary angle when the latitude, altitude, and declination are given, or the latitude when the horary angle is given. They might, however, be of excellent use in *single altitudes*, if they were about sixty times as extensive as they are.

In Mr. B.'s first paper, I cannot see how far he can conceive himself justified in endeavouring to depretiate the very valuable labours of Mr. Douwes and Dr. Brinkley, while at the same time he is pursuing a problem of a quite different and inferior nature, and which is no more than the declination, two altitudes of the sun, and the times from noon when those altitudes were taken, given to find the latitude.

Yours, &c.

Brompton, near Scarborough,
Feb. 13, 1826.

THOMAS BEVERLEY.

[Mr. Beverley proceeds at great length to the discussion of this problem, and states the mode of its solution as given in his forthcoming Mariner's Celestial Guide: but as so much has been said already upon the subject, we are desirous of bringing it to a close. We shall have great pleasure in hearing from Mr. Beverley on any other scientific subject, and are sure that he will not attribute our shortening his communication to any want of respect for the talent with which he has treated the subject.—EDIT.]

XXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 2.—A PAPER was read On the magnetizing power of the more refrangible rays of light; by Mrs. Mary Somerville: communicated by William Somerville, M.D. F.R.S.

The reading was commenced of a paper On the action of sulphuric acid upon naphthaline; by M. Faraday, Esq. F.R.S.

Feb. 9.—The reading of Mr. Faraday's paper was continued.

Feb. 16.—Mr. Faraday's paper was concluded: and a paper was read, On the circle of nerves which connects the voluntary muscles with the brain; by Charles Bell, Esq. F.R.S. E.

LINNÆAN SOCIETY.

Feb. 6.—Read, A description of the *Plectrophanes Lapponica*, a species lately discovered in the British Islands: by Prideaux John Selby, Esq. F.L.S. M.W.S. Ed. &c.*

Lapland Bunting (*Fringilla Lapponica* Linn.), *Emberiza calcarata* Temminck;— found in Leadenhall-market among Larks from Cambridgeshire. Fam. *Fringillidæ* Vigors. Gen. *Plectrophanes* Meyer. This genus Mr. Selby states to be intermediate between *Alauda* and *Emberiza*. It approaches the former in the thickness of the bill, and in the form of the feet and production of the hinder claw. Its affinity to *Emberiza* is shown in the peculiar form of the bill characteristic of that genus: it differs, however, in having the first and second quill-feathers nearly equal in length, and the longest in the wing.

Read also, Some account of a collection of Cryptogamic Plants formed in the Ionian Islands, and brought to this country by Lord Guildford. By Robert Kaye Greville, LL.D. F.R.S. E. &c.—Among the species described in this paper the following are new:—BYSSOIDEÆ; *Sporotrichum badium*, Thallus cæspitosus, badius: filis tenuissimis, confervoideis, implexis, sporidiis concoloribus, ovalibus, acervulis distinctis coacervatis.—GASTROMYCI; *Sclerotium gyrosum;* parvum, nigrum, erumpens, plano-convexum, sulcis gyrosis rugosum, intùs albidum.—ALGÆ; *Delesseria tenerrima*, fronde tenuissimâ, aveniâ, lineari, dichotomâ, roseâ, apice obtusâ, soris sporidiorum sparsis.—FUCOIDEÆ: *Zonaria rubra*, fronde reniformi, planâ, subintegerrimâ, fragili, nitidâ, rubrâ, lineis minutissimis longitudinaliter densissimè notatâ.—MUSCI; *Tor-*

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* Author of Illustrations of British Ornithology, a work of great merit; the very accurate plates of which are beautifully executed by Mr. Selby.

tula Northiana. Caulis brevis simplex, foliis erecto-patentibus, lineari-lanceolatis, acutis, siccitate tortuosis, theca subcylindrica (named after Lord Guildford.)—*Bryum elegans.*—*B. Donianum.*—*Hypnum Leskea.*

Feb. 21.—The Reading of Dr. F. Hamilton's Commentary on the *Hortus Malabaricus*, Part IV., was begun.

GEOLOGICAL SOCIETY.

Jan. 20.—A paper was read On the Geology of Jamaica, by H. T. De la Beche, Esq., F.R.S. &c.

Mr. De la Beche's observations are confined to the eastern half of Jamaica, which includes the whole range of the Blue Mountains, the highest eminences of the island, those of Port-Royal, Spanish-Town, the Mocko Mountains, and other ridges of inferior elevation. These heights often include or are connected with extensive plains, the principal of which are those of Liguanea, Vere, and Lower Clarendon, Luidas Vale, and St. Thomas's. The rocks of oldest formation which presented themselves to the author, within this district, he refers to the submedial or transition series. They compose the greater part of the Blue Mountain range, and consist of, 1. Gray-wacke, both foliated and compact, coarse and fine; presenting in short the usual variations common to this rock in Europe, and appearing, on some points, to pass into old red sandstone: 2. Transition limestone, apparently destitute of organic remains, compact, of a dark blueish gray colour, and traversed by veins of calcareous spar; occasionally associated with argillaceous slate, and its upper beds much intermixed with sandstones. These stratified rocks throughout the Blue Mountains generally dip towards the N.E. and E.N.E. at a considerable angle; but there are frequent exceptions to this rule, and the strata are on the whole much contorted. They are occasionally associated with trap rocks, viz. syenites, greenstones, and claystone porphyry. The author observed on one point, viz. the southern slope of St. Catherine's hill, a series of strata which he conceives to represent the coal measures; the old red sandstone is however developed on a larger scale, and in more numerous localities: so that the medial or carboniferous series is certainly not wanting in Jamaica. Resting upon this appears, on many points, a porphyritic conglomerate, associated with porphyry, and occasionally with greenstone and syenite. Similar trap rocks, intermixed in the most varied manner, show themselves very extensively, composing the greater part of the St. John's Mountains, and the district bordering on the Agua Alta. One variety of porphyry met with by the author is composed of nodular concretions, separated by a soft argillaceous substance, among which strings of chalcedony are sometimes found. It is remarkable, that the only instance of a similar structure which has occurred to the author, is in an amygdaloidal rock, decidedly of volcanic origin, at Black Hill, on another part of the island.

These trap rocks are found, generally, supporting the *great white limestone formation*, which occupies a very large portion of the whole island. This formation, from the fossils it contains, is referred by Mr. De la Beche to the tertiary series. It is principally composed of white limestone, most frequently very compact, and then strongly resembling the compact varieties of Jura limestone. The strata are usually very thick, varying from 3 to 20 feet in breadth. In some districts, this rock is interstratified with thick beds of red marle, and sandstone, and white chalky marle. The compact limestone constitutes the middle part of the formation: the lower beds consist, chiefly, of sands and marles, sometimes associated with blueish gray compact limestones, at others with beds of earthy yellowish white limestone, containing an abundance of organic remains, viz. *Echinites*, *Ostreæ*, and a particularly large species of *Cerithium*. The upper beds of the formation are rather chalky, sandy, and marly, and contain numerous remains of the genera *Conus*, *Cerithium*, *Astarte*, *Natica*, &c.; and near the sea coast a great quantity of corals, which, frequently, have almost a recent appearance.

Above the white limestone formation, beds of conglomerate and sandstone are visible on many points, particularly on the edges of the savannahs; whence the author calls them the *Savannah sandstones*.

The upper beds of all visible in the island, consist of *Diluvium* and *Alluvium*. The former shows itself on a very large scale, covering the surface of the principal plains, particularly that of Liguanea. It consists of rounded fragments of the rocks which compose the neighbouring mountains. The Hope river, which has cut its channel through the plain of Liguanea, has exposed sections of these diluvial gravel-beds, from 200 to 300 feet in thickness. The greater part of the large plain of Vere and Clarendon is also composed of diluvium. The pebbles of these beds consist chiefly of trap rocks; those of white limestone are comparatively rare, this rock appearing to have failed in resistance to the force of attrition by which its fragments were attacked. The separation between the diluvium and alluvium is not very decided; but deposits of the latter class have certainly been produced, in considerable quantities, along the course of many of the rivers; and on parts of the shore, particularly between Kingston and Port

Henderson, in front of which extends a long sand-bank, called the Palisades.

Mr. De la Beche's paper concludes with an interesting comparison of the Jamaica formations with those of Mexico and South America, as described by M. de Humboldt. The graywacke of Jamaica would seem to be continued in Mexico, with its accompanying trap rocks, and dark-coloured limestones. In South America it is absent; and its place is supplied solely by porphyries, syenites, and greenstones, which are developed there on a very large scale. The red sandstone which is found in Jamaica occurs very extensively in the neighbouring parts of the American continent. A formation analogous to the white limestone of Jamaica, seems, from M. de Humboldt's description, to occur both in Mexico and Venezuela.

Feb. 3.—A paper was read, entitled Remarks on some parts of the Taunus Mountains, in the duchy of Nassau; by Sir A. Crichton, V.P. G.S. &c. [An abstract of this paper will be given in our next.]

Feb. 17.—At the Anniversary Meeting of the Society held this day, the following gentlemen were elected Officers and Council for the year ensuing:

President: John Bostock, M.D. F.R.S.—*Vice-Presidents:* Sir Alexander Crichton, M.D. F.R. & L.S. *Hon. Memb. Imp. Acad. St. Petersburgh;* Rev. W. D. Conybeare, F.R.S.; Wm. Henry Fitton, M.D. F.R.S.; Cha. Stokes, Esq. F.R.A. & L.S. —*Secretaries:* W. J. Broderip, Esq.. F.L.S.; R. J. Murchison, Esq.; Tho. Webster, Esq.—*Foreign Secretary:* Hen. Heuland, Esq.—*Treasurer:* John Taylor, Esq. F.R.S.—*Council:* Arthur Aikin, Esq. F.L.S.; Henry Thomas De la Beche, Esq. F.R.S. & L.S.; J. E. Bicheno, Esq. Sec. L.S.: Henry Thomas Colebrooke, Esq. F.R.S. L. & E. F.L. & Asiat. Soc.; Sir Charles Henry Colvil; George Bellas Greenough, Esq. F.R. & L.S.; Sir Charles Lemon, Bart. F.R.S.; Armand Levy, Esq.; Cha. Lyell, Esq. F.R. & L.S.; William Hasledine Pepys, Esq. F.R.S. L.S. & H.S.: George Poulett Scrope, Esq.; J. F. Vandercom, Esq.; Henry Warburton, Esq. F.R.S.

ASTRONOMICAL SOCIETY.

Jan. 13.—There was read a paper by Stephen Groombridge, Esq., F.R.S., on the co-latitude of his observatory at Blackheath, as determined from his own observations. The author first describes a simple method of bringing the transit-instrument into the meridian, by the observations of Polaris and other circumpolar stars, and then by comparisons of high and low stars. He next describes the method of ascertaining the true zenith point, and thence the elevation of the pole, by observations of circumpolar stars in zenith-distance above and below the pole, from which twice the co-latitude becomes known. Employing his own constant of refraction, he obtains from observations of 32 circumpolar stars above and below the pole 77° 3′ 55″,65 for the mean double co-latitude; thence 38° 31′ 57″,82, and 51° 28′ 2″,18 for the latitude; a result which accords with his independent observations on the solstices.

Mr. Groombridge next proceeds to deduce from this, the co-latitude of the Royal Observatory. He determines the difference of the zeniths of the two observatories at 35″,25, which applied to the latitude of the Blackheath Observatory, by addition, gives 51° 28′ 37″,43 for that of the Royal Observatory, being less than Mr. Pond makes it by more than a second. Mr. Groombridge imputes the difference to an erroneous constant of refraction. The author concludes his paper, by presenting some simple formulæ for finding the position of a transit instrument, from the observed transits of a high and low star, passing the meridian to the south of the zenith; or from the observed transit of a circumpolar star above and below the pole.

There was next read, a communication from Sir Thomas Brisbane, dated Paramatta, 2d July, 1825. The contents were, 1st. Observations with a repeating circle for the winter solstice 1825, extending from June 12 to July 1 inclusive. These are not yet reduced. 2dly. Observations on the inferior conjunction of Venus and the Sun, May 1825, with the mural circle, from May 1st to the 25th inclusive. 3dly. Observations on the dip of the magnetic needle, March 1825;—the mean of the whole was 62° 41′ 35″. 4thly. Observations on the declination of the needle in March, April, and May, 1825;—the mean of the whole is 8° 59′ 48″. Lastly. An abstract of the meteorological Journal kept at Paramatta, from April 1824 to April 1825.

Feb. 10.—The Sixth Annual General Meeting of the Society was this day held at the Society's rooms in Lincoln's Inn Fields, for the purpose of receiving the Report of the Council upon the state of the Society's affairs, electing Officers for the ensuing year, &c. &c.

The President, F. Baily, Esq. in the chair.

From the Report, which was read by Dr. Gregory, we give the following extracts:

"In meeting the Astronomical Society of London at its Sixth Anniversary, the Council have great pleasure in being enabled still to use the language of cordial congratulation: for not only does the number of the Members and Associates of the Society

ciety continue to increase, and its affairs to prosper; but also the theory and practice of Astronomy (the extension of which was the sole object of the Society) have both been obviously promoted by the zeal and talent of many of its Members and friends."

The Report proceeds to state that "in 1822, the Members and Associates amounted to 188; in 1823, to 207; in 1824, to 210; in 1825, to 224; in February 1826, to 237;—a number, in which are included several of the most eminent promoters of Astronomy, not only in Britain but in Europe.

"Amongst the few Members of whom the Society has been deprived by death, the Council think it proper to call your attention to the loss of Mr. Cary. As an artist of considerable eminence and high reputation he was well known in the scientific world. Amongst the many excellent instruments which he contrived and perfected, he was the maker of the 2½-feet Altitude and Azimuth Instrument at Konigsberg, with which M. Bessel made his first observations at that celebrated Observatory.

"Among the duties, which it has devolved upon your Council to discharge, one of the most interesting has been the selection of papers (read at the ordinary Meetings) for publication in the volumes of the Memoirs of the Society. The Second Part of the First Volume, which was nearly ready for delivery at the Anniversary Meeting of 1825, was shortly afterwards laid before the public, and has been well received by Astronomers.—The First Part of the Second Volume is now nearly ready for publication: and the Council trust that it will experience an equally favourable reception. Besides several valuable papers tending to improve the theory of Astronomy and of astronomical instruments, and others describing instruments, which are entirely new; the several parts, here alluded to, contain tables, which tend very much to facilitate the labours of the practical Astronomer. Thus the second part of Vol. I. terminates with subsidiary Tables for facilitating the computation of annual tables of the apparent places of 46 principal fixed stars, computed by order of the Council; to which is prefixed a statement by the Foreign Secretary of the formulæ employed, and the elements adopted in their construction. These tables with their introduction occupy 76 pages.

"The Tables of precession, aberration, and nutation, serving to determine the apparent places of about 3000 principal fixed stars, to which allusion was made in the last Report of the Council, have been completed to 180° of Æ, and written out for the press. The remainder are in a state of considerable forwardness. These tables, together with an ample introductory paper on their construction and use, by the President of this Society, will constitute an appendix to the second volume of the Memoirs.

" Amongst the numerous communications which have been made from the Associates of this Society, the Council may specify a very interesting and elaborate paper, forwarded to the Foreign Secretary by M. Plana, on some important inquiries in physical Astronomy, which will be found in the second part of the second volume. The President also has received a letter from M. Bessel, requesting to know whether the Astronomical Society would patronize and promote a plan, which he had suggested, for making detached charts of the heavens. The President was requested by the Council to assure M. Bessel that the Astronomical Society would doubtless promote so laudable and useful a measure, as much as lay in their power. That active and indefatigable astronomer, pursuant to his general plan, now regularly observes all the smaller stars in zones, agreeably to the method suggested, and practised, by the late Rev. F. Wollaston. He has already completed the zones within 15° on each side of the equator; and in that space has observed upwards of 30,000 stars. The observations are annually published by M. Bessel, with the other observations made at the Royal Observatory at Konigsberg. When they are reduced (as there is great reason to hope they will be), they will constitute a most valuable accession to the stores of Astronomy.

" The instrument made use of in this survey of the heavens, as well as that used by Mr. Wollaston, were both made by the late Mr. Cary.

" Others of the Associates have especially distinguished themselves, and have forwarded to this Society some very interesting communications, as the successive parts and volumes of the Memoirs will evince. In alluding to these distinguished characters, your Council cannot avoid noticing the indefatigable labours of M. Schumacher, Professor of Astronomy at Copenhagen. His *Astronomische Nachrichten*, or Astronomical Newspaper, has considerably facilitated the intercourse between Astronomers in every part of the world; serving to record the observations of various interesting phænomena, as well as to draw the attention of observers to other phænomena about to appear. He has also published several compendious collections of tables of great practical utility. Among these, your Council cannot omit a particular reference to the very important Tables, which constitute the second part of his *Sammlung von Hülfstafeln*, and which have been prepared for the purpose of reducing the 50,000 stars contained in Lalande's *Histoire Céleste;* serving, indeed, to effect the reduction of any one of those stars in the short space of two or three minutes.

" Thus, whilst M. Schumacher has laid all Astronomers under considerable obligations by the publication of these tables, he has conferred a peculiar mark of his esteem upon the body now assembled, by dedicating this volume to the Astronomical Society; a distinction, which they, who know the talent and zeal of this our eminent Associate, will be able to appreciate in an adequate manner.

" One of our Associates, M. Struve, has devoted himself with great perseverance and success to the observation, and classification, of double stars; an important department of astronomical research, which was originally opened and pursued with his wonted assiduity and accuracy by our late revered president, Sir William Herschel.

" This subject has been still more extensively pursued, and with considerable ardour and zeal, by two of our Members, Messrs. Herschel and South; whose labours on this very interesting branch of the science are contained in a paper read before the Royal Society, and which in itself forms the third part of the Philosophical Transactions for the Year 1824. Whoever has read that paper with attention, must be struck with the vast labour and perseverance, the great accuracy and uniformity of result, with which those delicate observations have been made. Such an immense mass of interesting facts cannot fail to open new views to the contemplative philosopher, and extend our knowledge of the true system of the universe: and Mr. Herschel himself has, in a communication about to be laid before the Royal Society, made a happy application thereof, as explanatory of some of the phænomena connected with parallax. The indefatigable ardour of Mr. South in the cause of Astronomy, induced him to follow up his researches on the same subject whilst he was in France; and he has recently made a communication to the Royal Society, of some new observations, of equal, if not superior, importance; and which will appear in a subsequent volume of the Philosophical Transactions.

" For these laborious and valuable researches and observations relative to double stars, the Council have awarded to each of those distinguished Members and Associate, Mr. Herschel, Mr. South, and M. Struve, the Gold Medal of the Society, which will be presented to them at a General Meeting expressly called for that purpose, as soon as the medals can be prepared.

"Sir Thomas Brisbane, Governor of New South Wales, has devoted himself indefatigably to the practice of Astronomy, at Paramatta in that colony, having taken out with him some excellent instruments for that purpose. He and his assistants have already made several thousand observations, the records of which have been sent over to this country: and it is hoped that they will be published, either in their original shape, or after they have been reduced to some appropriate epoch. Dr. Brinkley, of Dublin, one of the Vice-Presidents of this Society, has instituted a series of computations on Sir Thomas Brisbane's Observations, with a view to the comparison of the results thus furnished, with the results deduced from observations made in the northern hemisphere. This particular inquiry has served to confirm the accuracy of the constant of refraction, formerly exhibited by that illustrious astronomer in his well-known formula for that species of reduction. Dr. Brinkley's paper on this subject is printed, and will appear in Part I. Vol. ii. of the Memoirs of this Society.

"Another of the Members of the Astronomical Society, the Rev. Fearon Fallows, Astronomer at the Cape of Good Hope, has also made a great number of Observations of the southern stars; and the Royal Society has published his Approximate Catalogue of 273 of the principal stars observed by La Caille.

"The continuance of Observations, such as these, at two Observatories in the southern hemisphere, cannot but be productive of considerable benefit to the science of Astronomy. In order, however, that they may be rendered subservient, in the highest degree, to the extension of this branch of knowledge, it is especially desirable that some efficient plan of co-operation should be arranged between the Astronomers at some of the northern Observatories, and those who are employed at the two above-mentioned stations, south of the equator. Those who are conversant with the history of Astronomy will recollect that when La Caille went to the Cape of Good Hope, in 1751, he addressed a circular letter to the principal Astronomers in Europe, enforcing the advantages of co-operation; and Lalande was in consequence sent to Berlin, to act in concert with him. Circumstances are now still more favourable for the production of advantageous results, provided a judicious plan of mutual co-operation be agreed upon. For while there is the Observatory established by Sir T. Brisbane in New South Wales, and that occupied by Mr. Fallows at the Cape; there are also in the northern hemisphere, M. Bessel at Konigsberg, M. Struve at Dorpat, and M. Argelander at Abö (the meridians of the four latter-mentioned places differing from each other but a very few degrees),—the respective

Astronomers, men of considerable science, activity and perseverance, and possessing instruments far superior to those, which were in existence in the time of La Caille. The advantages of this kind of pre-arranged co-operation, to which your Council here advert, are so well understood in the present advanced state of Astronomy, that a mere hint will (it is hoped) suffice, to produce the desired concert."

The Report then adverts to the contributions and exertions of other scientific bodies. "The erection of an Observatory at the University of Cambridge, and the still more recent announcement of a prize of 75*l.* at Edinburgh, to be awarded to the two best essays on Comets *, cannot but be hailed as of auspicious tendency in the developement of knowledge. In the same light, too, may doubtless be considered the determination of the British Board of Longitude, to employ adequate computers on the reduction of Mr. Groombridge's Observations at Blackheath, as well as to devote a part of the funds, which are at its disposal, to the arrangement and publication of the Observations of Tobias Mayer (so justly celebrated for their importance and accuracy) from the original manuscripts, which have been forwarded to this country for that express purpose.

"As another subject of congratulation, the Council cannot avoid noticing the interest which appears recently to have been excited in the United States of America to the subject of Astronomy. On the opening of the present Session of Congress, the President pointed out to them the propriety and advantage of constructing Observatories in various parts of their immense territory, and of establishing a system of co-operation between each other. A plan of this kind, under the direction of active and skilful Astronomers, cannot fail to advance the science, and is worthy of the patronage and protection of a great and powerful nation.

" No less than five comets were discovered within the compass of as many months in the last year, and one of these has (as it was predicted) been seen again within the last fortnight. This is a natural result of the augmented attention, which has been lately paid to these bodies, and to the investigation of the laws, which their motions obey.

" With respect to the Prize Questions proposed at the last general meeting of the Society, the Council report that they have received only one answer to the first question, which being just delivered in, is now under investigation. The period allotted for the determination of the second question will not expire till the next Anniversary, and that allotted for the third question not till the Anniversary in 1828: prior to which time the Council trust that the subjects proposed will have excited the attention of Astronomers, and induced them to forward to the Society the result of their inquiries and investigations.

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* Open to all students who have attended that University during the last ten years.

"It has frequently been a subject of regret with many Members of this Society, that there are so few particulars known relative to the different public Observatories in various parts of the world: such as the construction of the building, and the instruments with which it is furnished. The celebrated John Bernouilli in his *Lettres Astronomiques*, published at Berlin in 1771, attempted a description of some of those, which he had visited: but so many alterations have taken place since that period, not only in the Observatories themselves, (some of which no longer exist,) but also in the instruments, which are now of a totally new character, that but little information as to the present state of those establishments can be obtained from that source. The Council are of opinion that it would tend materially to the advancement of Astronomy, if an accurate description of every principal Observatory could be obtained, accompanied with a ground plan and elevation of the building; together with a description of the instruments employed, and drawings of such as are remarkable, either for their novelty or peculiar interest. It is well known that there are several instruments in constant use on the Continent, and much approved by Astronomers, which have not yet been seen in this country: and some in this country, which are not sufficiently known abroad; or even amongst ourselves. The Council would encourage every attempt to promote this species of information, by publishing in their Memoirs the accounts which they may from time to time receive on this subject, and the drawings, with which they might be accompanied.

"Your Council think it unnecessary to extend this Report to a greater length. It must be evident that many things, which (as far as regard the objects and labours of this Society) were six years ago only matters of hope and anticipation, have now become subjects of mutual congratulation. But it can only be by a cordial and zealous co-operation of all its Members, and by a continued course of perseverance, that the Society can ever expect fully to attain the principal objects for which it was established; and which, as stated in their original Address, are for the purpose of 'collecting, reducing, 'and publishing useful Observations and Tables:—for setting on foot a minute and systematic examination of the

' Heavens:—for encouraging a general spirit of inquiry in ' practical Astronomy:—for establishing communications with ' foreign Observers:—for circulating Notices of all remarkable ' Phænomena about to happen:—for enabling the public to ' compare the merits of different artists, eminent in the con- ' struction of astronomical instruments:—for proposing Prizes ' for the improvement of particular departments, and bestow- ' ing Medals or rewards on successful research in all:—and ' finally, for acting, as far as possible, in concert with every ' Institution both in England and abroad, whose objects have ' any thing in common with their own; but avoiding all inter- ' ference with the objects and interests of established scientific ' bodies.' Keeping these objects in view, as constant landmarks, the Council trust that the Society will insure the approbation and applause of every friend of science; and that it will not only prove a source of interest and information to the Members at large, but likewise tend to advance the progress of Astronomy in every habitable and civilized part of the globe."

After reading the Report and Treasurer's accounts, the Members proceeded to ballot for the officers for the ensuing year, when the following were declared to have been duly elected.

President: Francis Baily, Esq. F.R.S. L.S. & G.S.—*Vice-Presidents:* Rev. John Brinkley, D.D. F.R.S. Pres. R.I.A. *And. Prof. Ast. Univ. of Dubl.;* Capt. F. Beaufort, R.N. F.R.S.; Henry Thomas Colebrooke, Esq. F.R.S. L. & E. F.L.S. & G.S.; Davies Gilbert, Esq. M.P. V.P.R.S. F.L.S. & G.S—*Treasurer:* Rev. William Pearson, LL.D. F.R.S.—*Secretaries:* Olinthus G. Gregory, LL.D. *Prof. Math. Roy. Mil. Acad. Woolwich;* Lieutenant William S. Stratford, R.N.—*Foreign Sec.:* J. F. W. Herschel, Esq. M.A. Sec. R.S. Lond. & F.R.S. Ed.—*Council:* Colonel Mark Beaufoy, F.R.S. & L.S.; Benjamin Gompertz, Esq. F.R.S.; Stephen Groombridge, Esq. F.R.S.; James Horsburgh, Esq. F.R.S.; Daniel Moore, Esq. F.R.S.-S.A. L.S. & G.S.; John Pond, Esq. F.R.S. *Ast. Roy.*; Edward Riddle, Esq.; Richard Sheepshanks, Esq. M.A.; W. H. Fox Talbott, Esq. B.A.; Edward Troughton, Esq. F.R.S. L. & E.—The Society afterwards dined together at the Freemason's Tavern, to celebrate their sixth Anniversary.

ROYAL ACADEMY OF SCIENCES OF PARIS.

Sept. 5.—Doctors Sarmetaine, Flory, and Remonet, of Marseilles, announced, in a letter to the Academy, their intention of joining Dr. Costa and others, in submitting to all the experiments necessary to determine the question of the non-contagious or contagious nature of yellow fever.—Captain Vène communicated a memoir on circular functions.—M. Magendie presented some notes on the history of goitres, by Dr. Poulin of Santa-Fè-de-Bogotá.—MM. Legendre and Cauchy made a report on M. Berard's memoir in which he proposes to prove the truth of the only theorem of Fermet which has not yet been demonstrated.

Sept. 12.—M. Durville presented a MS. memoir on the Flora of the Malouine Isles.—M. Ampère communicated some new electro-dynamic experiments.—MM. Desfontaines and Labillardière made a report on M. Ad. de Jussieu's memoir on the family of the *Rutaceæ*.—M. Geoffroy St. Hilaire commenced the reading of a memoir entitled "On the beings of the intermediate degrees of the animal scale, which respire both in the air and under water, and which possess respiratory organs of two kinds, developed to a certain extent." He presented a specimen of the *Birgus Latro*, in which, besides *branchiæ*, there are organs which M. Geoffroy regards as *lungs*.

Sept. 19.—M. Geoffroy read another memoir in continuation, on the above subject.—M. Foulhious read a memoir on a law by which the arteries and nerves are governed in their respective relations.—M. Costa read a memoir on the epidemic typhus which ravaged the commune of St. Laurent-des-Ardens and its environs, during six months of 1823.—A memoir on the composition of new hydraulic morters, by M. Girard, was referred to a Committee.

Sept. 26.—M. Geoffroy St. Hilaire exhibited several living specimens of the common crab, *C. mænas*, and detailed verbally the results of his researches on the respiration of the *Crustacea*.

Oct. 3.—M. Féburier read an account of his experiments on the electricity of oxygen gas.—M. Ch. Gemmellaro communicated a memoir, in Italian, on the soil of Mount Ætna, with specimens in illustration.—MM. Quoy and Gaymard read some zoological observations on the Corals, made in the bay of Coupang, at Timor, and in the Isle of Guan, in the Mariannes.

Oct. 10.—M. Dulong read a memoir, entitled "Researches on the refractive powers of elastic fluids."—M. Lenoir, jun. read a memoir, by his father and himself, on the new instruments called *Levelling-circles*, which they have constructed.

Oct. 17.—M. Damoiseau read a memoir on the comet with a short period.—M. Dupetit-Thouars read a report on M. Gaudichaud's memoir respecting *Cycas circinalis*.—M. Geoffroy St. Hilaire read a memoir on a fœtal monster.

Oct. 24.—MM. Vauquelin and Thenard made a report on M. Laugier's memoir on the *Fer résinite* of Haüy, from Freyberg.—M. Geoffroy St. Hilaire read a memoir on the olfactory organs of fishes.—M. de Grandpré read a memoir on the means of sounding the ocean in order to discover the valleys which give rise to currents.

Oct. 31.—M. Serres communicated a work, in manuscript, on the comparative anatomy of animal monsters.—M. Moreau de Jonnès read some extracts from letters written from Martinique, detailing the ravages of the yellow fever and those of the last hurricane.

HORTICULTURAL AND AGRICULTURAL SOCIETY OF JAMAICA.

We feel much pleasure in announcing the establishment, on Jan. 10, 1825, of "The Society for the encouragement of Horticulture and of Agriculture, and of the arts connected with them, in Jamaica:"—the first, we believe, that has yet been formed in the British West Indies.

The following is a list of the Officers and Council of this Society.

Patron: His Grace William, Duke of Manchester, &c. &c. —*President:* Edward Nathaniel Bancroft, M.D., Fellow of the Royal College of Physicians, &c.—*Vice-Presidents:* Honourable John Mais; Samuel Murphy, Esq.—*Treasurer:* Robert Smith, Esq.—*Secretary:* John Miller, M.D.—*Honorary Members of the Council:* The Right Reverend the Lord Bishop of Jamaica; the Honourable William Anglin Scarlet, Chief Justice; the Honourable William Burge, Attorney-General.—*Council:* Honourable Joseph Barnes; Honourable Francis Smith; William Shand, Esq.; George Mills, Esq.; Edward Tichbone, Esq.; George Atkinson, Esq.; William Brooks King, Esq.; William Lambie, Esq.; Charles Mackglashan, jun. M.D.; James Wier, M.D.; Jacob Adolphus, M.D.; James Simpson, Esq.; Honourable James Laing; Sir M. B. Clare; John Lunan, Esq.; Stewart West, M.D.; William Gordon, M.D.; John Ferguson, M.D.; J. R. Phillips, Esq.; Thomas Higson, Esq.; C. S. Cockburn, Esq.; Rev. W. T. Paterson; Alexander M'Intosh, Esq.; Robert Gray, Esq.

The more especial objects of this association will be best seen from Nos. XI. and XII. of its regulations, with their subordinate heads, which are as follows:

"XI. That the following be the subjects for information, upon which prizes shall be offered; the communications to be sent to the Secretary by the 15th of November 1826:

“ 1. The progress and present state of agriculture in Jamaica or in the other West India colonies.

“ 2. The progress and present state of horticulture in Jamaica or in the other West India colonies.

“ 3. New methods by which the culture or preparation of the present staples of the island may be improved.

“ 4. The diseases of horses, mules, oxen, and sheep in the West Indies, and the means of curing them.

“ 5. The diseases of cultivated plants in this climate, and the modes of preventing and of checking them.

“ 6. The natural history of the insects, birds, and other animals, most destructive to vegetation, and the most effectual means of hindering or counteracting their ravages.

“ 7. The most œconomical modes of irrigating flat and mountainous lands, with the least waste of the nutritious particles of the soil.

“ 8. The most œconomical and effectual modes of draining marshy soils.

“ 9. Any valuable medical property in plants hitherto unknown.

“ 10. The preparation of wine from the vine (*Vitis vinifera*), and of vinous liquors from other fruits, in the Tropics.

“ 11. Descriptions of plants not previously known, or known imperfectly, with their botanical characters, and with specimens of each plant described, if practicable.

“ 12. The most advantageous modes of grafting in the Tropics, with an account of the plants on which these modes have been successful.

“ The Society shall likewise offer prizes for the following objects:

“ 13. Improved specimens of esculent vegetables and fruits, whether native or foreign, raised in this island.

“ 14. The introduction of any new and valuable plants, or esculent vegetables, or fruit. Specimens of each to be accompanied with an account of its history and cultivation.

“ 15. The best specimens of wines made within the Tropics, from the vine or from other fruits. Not less than three bottles of each sort of wine to be sent.

“ 16. To such persons of free condition, whether of colour or black, and male or female, as may, through his own industry, have put the cottage he has inhabited, with a garden attached to it, into the neatest condition, a premium not exceeding two doubloons.

"17. To a slave of either sex, for the same, a similar premium.

"XII. That the prizes to be bestowed by the society shall consist of silver medals of two sizes, and of premiums in money."

XXIII. *Intelligence and Miscellaneous Articles.*

DIFFERENCE OF LONGITUDE BETWEEN GREENWICH AND PARIS.

THE subjoined is a notice of Mr. Herschel's paper on this subject, read before the Royal Society on the 12th of January.

"An Account of a Series of Observations to determine the Difference of Longitude between the National Observatories of Greenwich and Paris; by J. F. W. Herschel, Esq. Sec. R.S.: communicated by the Board of Longitude."

In this paper, after stating the wish expressed by the French Ministry of War, that the above determination should be made, with the ready accession to their desire of our own Board of Longitude, and describing the method resorted to, Mr. Herschel gives the observations in detail. They were made by himself and one French officer on this side of the Channel, and by Capt. Sabine and another French officer on the coast of France. Their general result is $9' \, 21\frac{6}{10}''$ for the difference of longitude between the two Observatories; and though many of the observations had been rendered unavailable by untoward circumstances which it was impossible to foresee or to obviate, Mr. H. stated that this determination was not likely to require a correction exceeding 1-10th of a second, and very unlikely to want one of twice that amount.—*Ann. of Phil.*

EARTHQUAKE FELT AT SEA, IN FEBRUARY 1825.

There are few observations of greater importance, in reference to the theory of earthquakes, than the determination of the exact time when they are felt at sea. The place where they have their origin,—the velocity with which they are propagated,—and their probable depth beneath the surface, may be inferred from a series of accurate observations on the effects which they produce, and the time when they are felt at different points on the earth's surface.

The earthquake which was experienced at Lisbon, on the 2d February 1816, at five minutes past midnight, was felt at sea by the Portuguese vessel, the Marquis de Angeja, bound from Bengal to Lisbon, at the distance of 270 leagues from that city; and it was also experienced by another vessel, bound from Brazil to Portugal, at the distance of 120 leagues.

On the 4th of April 1812, the vessels on the coast of the Caraccas trembled, during the heavy shock of an earthquake, as if they had been on a reef of rocks.

In the earthquake which took place at Chili, on the 19th of November 1822, the effect on the ships in the bay was such, as if the chain-cable had run out in an instant.

On the 10th of February 1823, the East India Company's ship Winchelsea, in east long. 85° 33′, and north lat. 52°, experienced the effects of an earthquake. When the vessel was some hundred miles from land, and out of soundings, a tremulous motion was felt, as if it were passing over a coral rock, and this was accompanied with a loud rumbling noise, both of which continued for two or three minutes.

This effect bears a close resemblance to that which is described in the following extract of a letter from on board the Recovery, of ——, in a voyage from Madeira to Honduras, in February 1825.

"In running through among the islands, we were in dread of every schooner-rigged vessel we saw, as these seas swarm with pirates. However, nothing worthy of note occurred till off the island of Ruatan. Between seven and eight o'clock at night, being quite dark, we were all alarmed by a rumbling noise, as if the vessel had been running over a reef of rocks. Every one rushed upon deck, and all cast a wishful look over the side of the vessel, expecting every moment to see her go down. The pumps were sounded, but no water was in the well. It was then concluded, that it must have been a large log of timber which the vessel had come in contact with; but, on arriving in Belize, we ascertained that it was the effect of a smart shock of an earthquake, which had been experienced there at the very time we felt the concussion."—*Edin. Journ. of Science.*

FORMATION OF METALLIC COPPER BY WATER AND FIRE.

In making cement-copper in Germany, plates of *solid* copper are obtained, and also reguline copper in the fibrous, capillary, dentiform, reniform, and botryoid external shapes; and in the smelting of some sulphurets of copper, fibrous, lamellar, and crystallized pure copper is formed.—*Edin. Phil. Journ.*

EFFECT OF POSITION ON CRYSTALLIZATION.

Machman, professor of chemistry at Christiania, in Norway, in a memoir "On the Effect of the Earth's Magnetism on the Separation of Silver," states that in the year 1817, when exhibiting, in a syphon-shaped glass tube, the formation of an *arbor Dianæ*, the tube having accidentally been placed in the direction of the magnetic meridian, he remarked that finer and longer crystals were formed towards the north than towards the south, and yet every thing was the same in both legs of the tube. The solution of nitrate of silver in both legs of the tube was in communication, while the mercury covered only the bottom of the tube. The experiment was again repeated, in presence of Hansteen, with two syphon-tubes, one parallel, and the other at right angles to the magnetic meridian. The silver began to separate in the tube which was placed in the north and south direction, and shot out into larger, more numerous, and more brilliant radiations in the leg towards the north, than in that towards the south. In the syphon in the east and west direction no change was observed until the expiry of twelve hours. Hansteen afterwards repeated the experiment several times, and always with the same result, and deduced from his experiments the following inferences. 1. The *ar[illegible] Dianæ* is more strikingly developed when the tube is placed in the magnetic meridian, than when in the east and west direction. 2. When it remains in the magnetic meridian, the silver tree rises higher in the northern than in the southern leg. 3. The crystals are more acicular, and have a higher metallic lustre, in the northern than in the southern leg of the syphon. The same experiment has been successfully repeated by Dœbereiner and Schweigger, from whose Journal the above details are extracted.—*Edin. Phil. Journ.*

ACCOUNT OF PROFESSOR BERZELIUS'S METHOD OF DETECTING ARSENIC IN THE BODIES OF PERSONS POISONED.

Professor Berzelius has lately given some instructions for the discovery of arsenic in persons that have been poisoned with it. He considers the *reduction of arsenic to the metallic state as the only incontestible proof of the presence of this poison.* Arsenic may occur in two ways, viz. when it is found in substance (in the state of arsenious acid) in the dead body, and when it is not found in this state; though the intestines of the dead body may contain it in the state of a solution.

In the first of these cases, it is easy to determine the presence of arsenic. In order to do this, take a piece about three inches long of an ordinary barometer tube, and having drawn out one end of it into a much narrower tube, close the narrower end. Let some of the arsenic found in the body be now put in at the open wide end, so that it may fall down to the narrow end. Any quantity of this arsenic of sufficient volume to be taken from the body will suffice for this purpose. A little charcoal is then let fall upon the arsenic, after it has been freed from all moisture by bringing it to a red-heat with the blow-pipe. The charcoal is then heated in the tube at the flame of a spirit-lamp, the point of the tube being held out of the flame. When the charcoal is very red, the point containing the arsenic is drawn into the flame. The arsenic is then instantly volatilized, and passing into vapour by the red charcoal, it is reduced, and reappears on the other side of the flame in a metallic state. The flame is then brought slowly towards the metallic sublimate, which is thus concentrated into a smaller space in the small tube, and then presents a small metallic ring shining like polished steel*. We have now only to verify, by its smell, that the metallic sublimate is arsenic. For this purpose, cut the small tube with a file a little above the sublimate, and, having heated the place where it lies, put the nose above it at a small distance, and the particular odour of the metal will be immediately perceived.

In the case where the solid arsenic cannot be found, we must collect as much as possible of the contents of the stomach and the intestines, or even cut the stomach in pieces, and mix it with its contents. The whole is then to be digested with a solution of hydrate of potash. Hydrochloric acid is then added in excess. The whole is filtered, and, if the liquid is too much diluted, it is concentrated by evaporation. A current of sulphuretted hydrogen is then passed through it, which precipitates the arsenic in the form of the yellow sulphuret. If the quantity of arsenic is very small, the liquid will become yellow without giving a precipitate. It must then be evaporated, and in proportion as the hydrochloric acid becomes more concentrated, the sulphuret of arsenic will begin to be deposited. It is then filtered. If the sulphuret remaining on the filter is in too small a quantity to be taken from the paper, add some drops of caustic ammonia, which will dissolve it. Then put the liquid which passes the filter into a watch-glass, and evaporate it. The ammonia will be volatilized, and will leave as a residue the sulphuret of arsenic. If it shall still be difficult to collect the sulphuret, we must put into the watch-glass a little pulverized nitrate of potash, and, with the finger, mix the sulphuret with the nitrate of potash, which detaches it from the glass. At the bottom of a small phial, or a piece of glass tube, shut at one end, melt a little nitrate of potash at the flame of a spirit-lamp, and introduce into it, when melted, a little of the mixture which contains the sulphuret of arsenic. It is oxidized with effervescence, but without fire, or detonation, and without loss of arsenic. The melted salt is then to be dissolved in water, and lime added in excess, and the liquid boiled. The arseniate of lime will then be deposited, and may be collected. When dried, it is mixed with charcoal, and then brought to a red heat by the blowpipe, and a small quantity of this mixture is allowed to fall to the narrow end of the above tube. It is now gradually heated to expel all humidity which tends to throw it into the wide tube, and when it is very dry, heat at the flame of the blowpipe, the part of the tube which contains the mixture. The arsenic will be disengaged, and be sublimed, at a distance from the heated part. An addition of vitrified boracic acid greatly promotes the decomposition which then takes place at a less elevated temperature; but this acid frequently contains water, and produces a bubbling of the melted matter which raises it in the tube, and causes the vapours to issue by perforating the softened part of the glass.

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* Had the experiment been made in the wide part of the tube, the result would scarcely have been visible with a small quantity of arsenic.

M. Berzelius maintains, that the *sixth part of a grain of sulphuret of arsenic is sufficient to make three different trials;* but he adds, that, when we have discovered only very small traces of arsenic, we must take care not to introduce any by means of re-agents, among which both the sulphuric and the hydrochloric acid may contain it. The first almost always contains some arsenic when it is not manufactured from volcanic sulphur; and the second, in consequence of sulphuric acid being used in the preparation of the hydrochloric acid, yields the arsenic which it contains in separating it from soda. We must, therefore, be certain of the purity of these re-agents.

When death has been caused by the arsenic, and not by the arsenious acid, the process must be modified, because the sulphuretted hydrogen gas decomposes the arsenic acid too slowly. In this case, we must add hydrosulphuret of ammonia, which reduces the arsenic acid to the state of sulphuret, which is afterwards precipitated by the hydrochloric acid.—*Edin. Journ. of Science.*

ON THE COMBUSTION OF COMPRESSED GAS.—BY MR. DAVIES.

In making, upwards of twelve months ago, some experiments upon the combustion of compressed gas, I accidentally observed a fact which is, I think, of rather a singular nature.

When the aperture of the burner is, in this case, too large, the flame cannot be maintained, being blown away by the rapid current of the gas. When it is rather small, the flame is under the best circumstances. If the aperture be further enlarged without being carried to the extent at which the combustion is extinguished, the flame will then be blue, noisy, and agitated, affording very little light. But I found, to my great surprise, that if, when the flame was in this last state, the vessel of the gas was inverted, the flame was instantly changed; and instead of being as I have just stated, it was steady, silent, and powerful. I have repeated the experiment frequently, and with different vessels. In every instance the result has been precisely the same.

It became interesting to inquire into the cause of the phænomenon. I submit with deference the only explanation which I have been able to discover.

The gas, rarefied by heat, being lighter than the atmosphere, has a tendency to move in the direction of the flame when the vessel is held upright. In this case, therefore, it moves with greater impetuosity than it could were the burner in any other position. On the contrary, when the flame is directed downwards, it has a tendency to return upon itself. Thus the ascent of the gas is promoted, and the descent retarded, by the agency of the atmosphere; for the gas being rendered lighter in the way just mentioned, has a tendency to rise in the air on the same principle that a cork rises in water, and its descent is in like manner resisted. The fact might, perhaps, be better illustrated by conceiving air to be forced through water. If the air be urged from the bottom of the vessel, it readily moves by reason of its great levity in the required direction; but if it be forcibly impelled downwards from the surface, as from the extremity of a condensing syringe, it can only be driven to a short distance, and it is then forced back towards the pipe. This case appears to me to be analogous to that of the gas, which, if I am not mistaken, it serves to illustrate and explain. The upright position of the vessel admits, in the case referred to, of the escape of some of the gas unburnt; but when the burner is inverted, the flame, for reasons already assigned, returns upon the stream of gas, and the combustion, which was before imperfect, is then complete.

How far the fact may be susceptible of a practical application, I am not at present prepared to offer an opinion; but the consumption of the gas is, by this mode of burning, very considerable, and I have not yet been able to determine that there is in the combustion of gas under the ordinary pressure, any increase of illuminating power obtained by inverting the burner. —*Annals of Philosophy.*

ON THE INVISIBILITY OF CERTAIN COLOURS TO CERTAIN EYES.

A variety of cases have been recorded, where persons with sound eyes, capable of performing all their ordinary functions, were incapable of distinguishing certain colours; and what is still more remarkable, this imperfection runs in particular families. Mr. Huddart mentions the case of one Harris, a shoemaker at Maryport in Cumberland, who could only distinguish black and white, and he had two brothers almost equally defective, one of whom always mistook orange for green. Harris observed this defect when he was four years old, and, chiefly from his inability to distinguish cherries on a tree like his companions. He had two other brothers and sisters, who, as well as their parents, had no such defect. Another case of a Mr. Scott is recorded in the Philosophical Transactions, in which full reds and full greens appeared alike, while yellows and dark blues were very easily distinguished. Mr. Scott's father, his maternal uncle, one of his sisters, and her two sons, had all the same imperfection. Our celebrated chemist, Mr. Dalton, cannot distinguish blue from pink by daylight; and in the solar spectrum the red is scarcely visible, the rest of it appearing to consist of two colours, yellow and blue. Dr. Butters, in a letter addressed to the editor of this work, has described the case of Mr. R. Tucker, son of Dr. Tucker of Ashburton, who mistakes orange for green, like one of the Harrises. Like Mr. Dalton, he could not distinguish blue from pink; but he always knew yellow. The colours in the spectrum he describes as follows:

1. Red mistaken for . . . . brown,
2. Orange . . . . green,
3. Yellow, generally known, but sometimes taken for orange,
4. Green mistaken for . . . . orange,
5. Blue . . . . pink,
6. Indigo . . . . purple,
7. Violet . . . . purple.

Mr. Harvey has described, in a paper read before the Royal Society of Edinburgh, and which will soon be published, the case of a person now alive, and aged 60, who could distinguish with certainty only white, yellow, and gray. He could, however, distinguish blues when they were light. Dr. Nichols has recorded a case where a person who was in the navy purchased a blue uniform coat and waistcoat, with *red* breeches to match the *blue;* and he has mentioned one case in which the imperfection is derived through the father, and another in which it descended from the mother.

In the case of a young man in the prime of life, with whom the writer of this article is acquainted, only two colours were perceived in Dr. Wollaston's spectrum of five colours, viz. red, green, blue, and violet. The colours which he saw were *blue* and *orange* or *yellow*, as he did not distinguish these two from one another. When all the colours of the spectrum were absorbed by a reddish glass, excepting *red* and *dark green*, he saw only one colour, viz. yellow or orange. When the middle of the red space was absorbed by a blue glass, he saw the black line with what he called the *yellow* on each side of it. We are acquainted with another gentleman who has a similar imperfection.

In all the preceding cases there is one general fact, that *red light, and colours in which it forms an ingredient, are not distinguishable by those who possess the peculiarity in question.* Mr. Dalton thinks it probable that the red light is, in these cases, absorbed by the vitreous humour, which he supposes may have a blue colour: but as this is a mere conjecture, which is not confirmed by the most minute examination of the eye, we cannot hold it as an explanation of the phænomena. Dr. Young thinks it much more simple to suppose the absence or paralysis of those fibres of the retina which are calculated to perceive red; while Dr. Brewster conceives that the eye is, in these cases, insensible to the colours at the one end of the spectrum, just as the ear of certain persons has been proved, by Dr. Wollaston, to be insensible to sounds at one extremity of the scale of musical notes, while it is perfectly sensible to all other sounds.

If we suppose, what we think will ultimately be demonstrated, that the choroid coat is essential to vision, we may ascribe the loss of red light in certain eyes to the retina itself having a blue tint. If this should be the case, the light which falls upon the choroid coat will be deprived of its red rays, by the absorptive power of the blue retina, and consequently the impression conveyed back to the retina, by the choroid coat, will not contain that of red light.—*Edin. Journ. of Science.*

ON THE POISON OF THE COMMON TOAD. BY DR. J. DAVY.

The following is an abstract of Dr. Davy's paper on this subject, lately read before the Royal Society.

The popular belief in the venomous nature of the toad, Dr. Davy states, though of great antiquity, has been rejected as a vulgar prejudice by modern naturalists, decidedly so by Cuvier; but like many other long received and prevalent opinions, it is a true one, and the denial of it by philosophers has resulted from superficial examination. Dr. D. found the venomous matter to be contained in follicles, chiefly in the cutis vera, and about the head and shoulders, but also distributed generally over the body, and even on the extremities. On the application of pressure this fluid exudes, or even spirts out to a considerable distance, and may be collected in sufficient quantity for examination. It is extremely acrid when applied to the tongue, resembling the extract of aconite in this

respect,

respect, and it even acts upon the hands. It is soluble, with a small residuum, in water and in alcohol, and the solutions are not affected by those of acetate of lead and corrosive sublimate. On solution in ammonia, it continues acrid; it dissolves in nitric acid, to which it imparts a purple colour. By combination with potash or soda it is rendered less acrid; apparently by partial decomposition. As left by evaporation of its aqueous or alcoholic solutions, it is highly inflammable; and the residuary matter that appears to give it consistence seems to be albumen. Though more acrid than the poison of the most venomous serpents, it produces no ill effect on being introduced into the circulation; a chicken inoculated with it was not affected.

The author conjectures that this "sweltered venom," as it is correctly termed by our great dramatist, being distributed over the integuments, serves to defend the toad from the attacks of carnivorous animals;—"to eat a toad" has long been held as an opprobrious difficulty; and the animal is still further protected in this respect by the horny nature of its cutis, which contains much phosphate of lime, &c. As the venom consists in part of an inflammable substance, it is probably excrementitious, and an auxiliary to the action of the lungs in decarbonizing the blood. This view of its use is confirmed by the fact that one of the two branches of the pulmonary artery supplies the skin, its ramifications being most numerous where the follicles of venom are thickest.

Dr. Davy has found the skin of the toad to contain *pores* of two kinds: the larger, chiefly confined to particular situations, and which, when the skin is held up to the light, appear as iridescent circles, and the smaller, more numerously and generally distributed, which appear as luminous points of a yellowish colour. Externally these pores are covered with cuticle, and some of the larger ones even with rete mucosum; internally they are lined with delicate cellular tissue. By inflating the skin, Dr. D. ascertained that it was not furnished with spiracula, the existence of which he had been led to suspect by some particular circumstances in the physiology of the animal. —*Ann. of Phil.*

LIST OF NEW PATENTS.

To Robert Rigg, of Bowstead Hill, Cumberland, for a new condensing apparatus, to be used with the apparatus now in use for making vinegar.—Dated 4th February 1826.—6 months to enrol specification.

To J. C. Gamble, of Liffeybank, in the county of Dublin, chemist, for his apparatus for the concentration and crystallization of aluminous and other saline and crystallizable solutions,

tions, part of which may be applied to the purposes of evaporation, distillation, inspissation, and to the generation of steam. —7th February.—4 months.

To William Mayhew, of Union-street, Southwark, and William White, of Cheapside, for their improvement in the manufacture of hats.—7th February.—6 months.

To Hugh Evans, Harbour-master, of Holyhead, for his method of rendering vessels, whether sailing or propelled by steam, more safe in cases of danger by leakage, &c.—7th Feb. —2 months.

To William Chapman, of Newcastle-on-Tyne, for his improved machinery for loading or unloading of ships.—7th February.—2 months.

To Benjamin Cook, of Birmingham, brass-founder, for improvements in making files.—7th February.—6 months.

To William Warren, of Crown-street, Finsbury-square, for improvements (communicated from abroad), in the process of extracting from the Peruvian bark quinine and cinchonine, and preparing the various salts to which these substances may serve as a basis.—11th February.—6 months.

To John Lane Higgins, of Oxford-street, for improvements in the construction of the masts, yards, sails, rigging of ships, and in the tackle used for navigating the same.— 11th February. — 6 months.

To Benjamin Newmarch, of Cheltenham, and Charles Bonner, of Gloucester, for their invention for suspending and securing windows, gates, doors, shutters, blinds, and other apparatus.—18th February.—6 months.

To Thomas Walter, of Luton, Bedfordshire, for improvements in straw plats, for making hats, &c.—18th February.— 6 months.

To Charles Whitlaw, of Bayswater Terrace, Paddington, for his improvement in administering medicines by the agency of steam.—18th February.—6 months.

To Arnold Buffum (late of Massachusetts, but now of Bridge-street, London), for improvements (in part communicated from abroad) in making and dyeing hats.—18th Feb.—6 months.

Results of a Meteorological Journal for January 1826, *kept at the Observatory of the Royal Academy, Gosport, Hants.*

General Observations.

The first part of this month was fair and frosty, with the exception of two or three days; and the latter part very damp and humid, with variable winds from the east side of the meridian. The frosty weather was ushered in by a N.E. wind, which blew strong from that point nearly seven days; it then shifted to the N. and N.W. with a low temperature till the 13th instant, which was the coldest day and night we had had since the 15th of January 1820. Here the thermometer in the external air at 7 o'clock A.M. on the 14th, was as low as 17 degrees: in London, on the morning of the 16th, it sank to 15 degrees; and in Paris it was said to have receded several degrees lower. In the morning of the 8th all the pumps that were not under cover were ice-bound, and continued so nine or ten days. On the 9th, the skaters assembled upon the ice in Stoke's Bay-Marsh, and upon the Moat round the Fortification of Gosport, where they amused themselves and the bystanders eight or nine days; as the calm, clear, and frosty weather afforded a favourable opportunity. In the morning of the 15th there was an apparent change in the atmosphere, when three winds prevailed simultaneously; viz. the lower one from the E., the next from the S.E., and the upper one from N.W., with a rising temperature, which continued till the 21st, when the external thermometer rose to 46 degrees: but the lower wind being dry, the barometer rose steadily till the evening of the 17th; and on the 18th the frost went off, succeeded by drizzling rain. A more favourable thaw could not have been desired, as it was remarkably gradual, attended with scarcely sufficient rain to wet the ground; and the thick masses of ice had not entirely dissolved into a fluid state till the close of the month. Here we had not enough snow to cover the ground; but in the northern parts of the country it was several feet in depth, so that the stage coaches could not pass for some days. In Paris too it was nearly a foot in depth, which was deeper than had been known there for some years past. The change in the atmosphere, from a very cold and dry state to a considerable increase of temperature and great dampness, was attended, as usual, with colds, coughs, and rheumatism. The weather, however, was seasonable and healthy till the full of the moon.

A few minutes after sunset on the 9th, there appeared round the horizon a dark purple haze with an even altitude of about 5 degrees; next to this was a band of red 2½ degrees wide, surmounted by a band of yellow of the same width. The primitive colours thus forming contiguous bands near the horizon, but brighter diametrically opposite to the sun, had a fine appearance, and were produced by reflection of the sun's horizontal rays from the falling frozen dew or descending hoar frost.

The atmospheric and meteoric phænomena that have come within our observations this month are one parhelion, one solar and three lunar halos, two meteors, and six gales of wind, or days on which they have prevailed, from the N.E.

Numerical Results for the Month.

		Inches.			
Barometer	Maximum	31·51,	January 17th	—Wind	S.
	Minimum	29·54,	Ditto 6th	—Ditto	N.E.

Range of the mercury . . 0·97.

	Inches.
Mean barometrical pressure for the month	29·980
——— for the lunar period ending the 8th inst. . .	29·693
——— for 14 days, with the Moon in North declin.	29·671
——— for 16 days, with the Moon in South declin.	29·715
Spaces described by the rising and falling of the mercury	3·550
Greatest variation in 24 hours	0·340
Number of changes	21

Thermometer	Maximum	49°,	January 31st	—Wind	S.W.
	Minimum	17	Ditto 14th	—Ditto	E.

Range	32
Mean temp. of the external air	35·56
——— for 29 days with the Sun in Capricornus . . .	34·21
Greatest variation in 24 hours	17·00
Mean temp. of spring water at 8 o'clock A.M. . . .	50·10

De Luc's *Whalebone Hygrometer.*

	Degrees.
Greatest humidity of the air .	93 in the evening of the 30th.
Greatest dryness of ditto . . .	59 in the afternoon of the 9th.
Range of the index	34
Mean at 2 o'clock P.M. . . .	74·0
—— at 8 o'clock A.M. . . .	80·1
—— at 8 o'clock P.M. . . .	78·9
—— of three observations each day at 8, 2, and 8 o'clock	77·7

Evaporation for the month	1·000 inch.
Rain in the pluviameter near the ground .	0·890
Rain in ditto 23 feet high	0·825

Prevailing winds, N.E.

A Summary of the Weather.

A clear sky, 5; fine, with various modifications of clouds, 10; an overcast sky without rain, 12; foggy, 1; rain, 3.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
11	8	27	1	9	20	13

A Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
3	10	3	4	3	2½	½	5	31

A METEOROLOGICAL TABLE: *comprising the Observations of Dr.* BURNEY *at Gosport, Mr. J.* CARY *in London, and Mr.* VEALL *at Boston.*

Days of Month, 18[illegible]6.	[illegible]port, at half-past Eight o'Clock, A.M.							Clouds.							Height of Barometer, in Inches, &c.		Thermometer.				Rain.		Weather.		
	Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	[illegible]	Evaporation.	Rain near [illegible]	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Lond. 1 P.M.	Bost. 8½ A.M.	London. 8 A.M.	London. Noon.	London. 11 P.M.	Boston. 8½ A.M.	London.	Boston.	London.	Boston.	Wind.
☾ Jan. 1	29·73	39	51·35	83	SW.	...	0·020	...	...	1	...	...	1	1	29·82	29·58	39	41	[illegible]	33	...	[illegible]	Cloudy	Cloudy	SE.
2	29·70	42	......	82	[illegible]	...	......	1	1	1	...	...	1	...	29·87	29·64	38	41	34	37·5	...	...	Fair	Fine	SE.
3	29 75	39	......	77	[illegible]	0 08	......	1	...	1	...	...	1	...	29·90	29·77	33	35	34	37	...	...	Fair	Fine	W.
4	29 77	32	......	75	[illegible]	...	......	1	1	1	...	...	...	...	29·91	29·82	34	36	[illegible]	34·5	...	...	Cloudy	Cloudy	SW.
5	29·71	32	[illegible]	87	NE.	...	·750	...	...	1	...	...	1	1	29 87	[illegible]	[illegible]	[illegible]	[illegible]	38	...	[illegible]	Sleet	Rain	NE.
6	29·56	36	......	88	NE.	·07	·060	...	...	...	...	...	1	1	29·77	[illegible]	[illegible]	[illegible]	[illegible]	38	...	·09	Cloudy	Stormy	NE.
7	29·70	36	......	87	NE.	...	......	...	...	1	...	...	1	...	29·83	29·70	[illegible]	34	34	37·5	...	...	Cloudy	Cloudy	NE.
● 8	29·78	31	......	61	NE.	...	......	...	...	...	...	1	1	1	29·76	29·84	31	32	28	32	...	...	Fair	Cloudy—snow *p.m.*	W.
9	29·88	[illegible]	......	67	NE.	·10	......	...	...	1	...	...	1	...	29·99	29 93	26	27	24	29·5	...	...	Fair	Cloudy	SE.
10	29·76	24	50·60	71	N.	...	......	1	1	1	...	...	1	1	29·76	29·67	24	20	28	17	...	...	Fair	Fine	NE.
11	29·65	29	......	77	N.	...	......	...	...	1	...	1	...	...	29·76	29·60	29	32	25	30	...	...	Snow	Cloudy	N.
12	29·73	23	......	80	[illegible]	·10	......	...	...	1	...	1	...	...	29·72	29·60	24	28	26	23	...	...	Fair	Fine	N.
13	29·90	20	......	81	NW.	...	......	...	...	...	...	1	...	...	30·00	29·77	18	29	24	17	...	...	Fair	Fine	NW.
14	29·98	21	......	79	E.	...	......	1	1	1	...	1	1	1	30·10	29 92	19	26	24	16	...	...	Fair	Fine	NW.
15	30 10	29	......	80	E.	·07	......	1	1	1	1	1	1	...	30 27	30 13	21	23	18	17	...	...	Foggy	Cloudy	NW.
☽ 16	30·40	29	......	79	E.	...	......	...	...	1	...	1	1	1	30·53	30 43	15	27	24	10	...	...	Foggy	Fine	NW.
17	30·50	32	50·00	76	S.	...	......	1	1	1	...	1	...	...	30·59	30·43	23	32	29	26 5	...	. .	Fair	Cloudy	S.
18	30·44	34	......	78	S.	03	·010	1	1	1	...	...	...	1	30·40	30 30	31	32	39	31	...	·03	Fair	Cloudy	S.
19	30 10	42	......	85	NW.	...	......	...	...	1	...	...	1	...	30·15	29·88	40	42	39	37	...	...	Fair	Fine	W.
20	30·16	38	......	85	NW.	...	......	...	...	1	...	...	1	...	30·23	30·00	40	39	38	36	...	...	Cloudy	Fine	W.
21	30·15	40	49·80	83	S.	·05	·005	1	...	1	...	...	1	1	30·18	29·95	38	39	37	37 5	...	...	Rain	Cloudy	S.
22	30·10	39	......	84	N.	...	015	...	...	1	...	...	1	1	30·26	29·95	39	34	34	35	...	...	Cloudy	Fine	W.
23	30·15	39	......	85	N.	...	·010	...	...	1	...	...	1	1	30·23	29·83	33	37	37	35	...	...	Rain	Cloudy	SW.
○ 24	30·34	38	......	84	NE.	·10	......	...	...	1	...	...	1	...	30·42	29·90	33	34	33	30	...	...	Foggy	Fine	W.
25	30·26	38	.. ...	83	NE.	...	......	...	...	1	...	...	...	...	30·34	30·00	35	34	34	32 5	...	...	Cloudy	Cloudy	W.
26	30·21	34	49·60	79	NE.	...	......	...	...	1	...	1	...	...	30·33	30·17	33	35	30	33 5	...	...	Cloudy	Cloudy	NE.
27	30·18	33	......	82	NE.	·15	......	...	...	1	...	...	1	...	30 30	30 10	29	36	32	33	...	...	Fair	Cloudy	W.
28	[illegible]	35	......	80	E.	...	......	...	...	...	...	...	...	...	30·25	30·03	32	39	32	31	...	...	Fair	Cloudy	W.
29	[illegible]	38	......	80	S.	...	......	1	...	1	...	...	...	...	[illegible]	29 88	32	43	38	32 5	...	...	Fair	Fine	S.
30	[illegible]	41	......	82	S.	...	·010	...	...	1	...	...	1	1	[illegible]	29.60	40	45	43	36·5	...	...	Cloudy	Cloudy—rain *p.m.*	SE.
31	[illegible]	43	49·25	83	SW.	·25	·010	1	1	1	...	...	...	1	29 80	29·47	44	45	43	42	[illegible]	0 8	Cloudy	Cloudy	S.
Aver.:	29·978	33·87	50·10	80·1		1·00	0·890	11	8	27	1	9	20	13	30·10	29 88	32	35	33	30·5					

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XXIV. *On the Figure of the Earth.* *By* WILLIAM GALBRAITH, *Esq. M.A.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

IN some of the previous Numbers of your Journal I was induced to make a few remarks on several of the most accurate systems of experiments by the pendulums hitherto executed for the purpose of determining the figure of the earth. Since that time Captain Sabine's work on that subject has appeared, containing an extensive series of experiments in the northern hemisphere, reaching from the equator to about 80° north latitude, embracing the longest arc of the meridian yet attempted; and executed, it is believed, with an accuracy which cannot easily be surpassed. Capt. Sabine has also reconsidered some of the experiments of others, particularly those made by the French mathematicians on the arc passing chiefly through France; and, by applying corrections analogous to those employed by himself, has by that means rendered them more consistent, and comparable with each other, and with his own. By the French experiments it appeared from the character of the errors,—Phil. Mag. vol. lxiv. p. 167, but more especially from vol. lxv. p. 15,—that the gravitating force at about 45° N. was less from experiment than theory required, so far as the accuracy of these experiments could be depended upon; or in other words, that the pendulum was more distant from the centre of the earth than could have been anticipated: and this conclusion seemed to derive some support from the compression obtained from the measurement of arcs near the mean parallel.

Whether this conclusion, which it must be admitted is not very natural, is to be ranked among those views which the early French mathematicians entertained, in opposition to Newton, remains to be determined. Perhaps the nature of the ground over which the arc passed, when properly examined, would, in part at least, explain the irregularity in question; since at Formentera the pendulum was not much above the level of the sea, in the interior of France it was considerably elevated, and again at Dunkirk it descended nearly to the level of the ocean. Now it is obvious, that if the experimental results were not properly reduced to what they would have been at the level of the sea, the configuration of the country would have, generally speaking, produced the irregularity we have endeavoured to point out. May not this also have its effect on the compression derived from the measurement of arcs? It is probable it would to a certain extent. Indeed it appears certain, that irregularities of this nature must be expected, unless regularity of ground, and similarity of geological character, be selected for either of these series of experiments; and this, it must be granted, cannot easily be obtained, though it should be attended to as far as circumstances will permit. The truth of these remarks will be obvious from a comparison of Captain Sabine's observations at St. Thomas and Ascension, with those at Maranham and Trinidad.

Captain Sabine has combined all his own observations with some of those of Captain Kater and of the French, and allows each to have its proper share in determining the coefficients of the theoretic formula, as well as the compression; and in general this method is to be recommended, where solid objections cannot be made to some particular observations. Now in the present case, we think strong objections may be urged against some of them; particularly those on basaltic or volcanic bases, as those at St. Thomas, Ascension, Galapagos, &c. being combined with others on alluvial soils.

It is true we have three determinations of the length of the pendulum, when nearly on the equator: one at Galapagos, one at St. Thomas, and another at Java; though the temperature at which this last was determined is not mentioned in the source whence we obtained it. A mean of all these would give about 39·02 inches for the length of the equatorial pendulum; but, unfortunately, two of them at least were obtained on rocky bases, and may therefore be considerably more than when determined on a basis of an ordinary state of geological character. Under these circumstances it would perhaps be prudent to reject those which are obviously affected by such a cause, and by means of the usual formulæ to reduce a considerable number of observations near the point where we wish to obtain it with great precision, to that point exactly. The observations are recommended to be near it, in order, as much as possible, to avoid an error arising from any small error in the coefficients of the general formula. Proceeding on these principles, it may be supposed that the deviation from the truth would be nearly insensible.

But accurate observations on the length of the pendulum at stations selected with judgement, are not of themselves the only data necessary to determine the compression. If there is any error in the fundamental formula employed for this purpose, a corresponding error will be communicated to the amount of compression. It is true that the deductions hitherto given have the appearance of great accuracy; for when expressed by a fraction of which the numerator is unit, the denominator is frequently carried to one or two places of decimals. This, however, is a mere deception, if it can be shown that the denominator of that fraction is by the ordinary formula erroneous to the amount of several units. The formula first demonstrated by Clairaut has been acquiesced in by Laplace, Delambre, Borda and Biot in France, and by Kater and Sabine in England. Now it is well known that it is but an approximation obtained in course of the analysis by omitting the powers greater than the first. "At the present time," says Mr. Ivory (Phil. Mag. vol. lxvi. p. 432), one of the ablest geometers of the age, "when so much has been done, and is still doing, to determine the figure of the earth experimentally, it seems proper likewise to reconsider the theory." With these sentiments our opinion perfectly coincides. The failure of Clairaut's first attempts to integrate a differential equation in the solution of the famous problem of the three bodies in his theory of the moon, is well known to geometers, and might have suggested the propriety of examining this celebrated theorem, and to determine the degree of its accuracy by taking in at least another term comprehending the squares in the series expressing the ratio of the centrifugal force to gravity. We intended originally to have given a complete analysis of this theorem from first principles; but since the time which we are enabled to devote to such speculations is but limited, we shall content ourselves by referring to a very masterly paper by Mr. Ivory in the Philosophical Transactions for 1824. It is there demonstrated that

$$q = \frac{2}{5} \sin^2 \varphi + \frac{2}{35} \sin^4 \varphi - \&c. \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

Now when the oblate spheroids do not differ very considerably from spheres, as in the case of the planets; λ, which is equal to the eccentricity of the meridian divided by half the polar axis is so small that we may consider λ^2 as equal to $\sin^2 \phi$ &c., and in this case $q = \frac{2}{5}\lambda^2 + \frac{2}{35}\lambda^4 \quad . \quad . \quad . \quad . \qquad (2)$

Now by reversion of series $\lambda^2 = \frac{5}{2} q - \frac{25}{28} \lambda^4 \quad . \quad . \qquad (3)$

But the polar axis is to the equatorial diameter as 1 to $\sqrt{1+\lambda^2}$, or 1 to $1 + \frac{1}{2}\lambda^2 - \frac{1}{8}\lambda^4$ &c. And by substituting for λ^2 its value from equation (3) we have

$$\sqrt{1+\lambda^2} = \sqrt{1 + \frac{5}{2}q - \frac{25}{28}q^2} = 1 + \frac{5}{4}q - \frac{275}{224}q^2 \text{ \&c.}$$

Hence $1 + \frac{5}{4}q - \frac{275}{224}q^2 = 1 + \frac{1}{2}\lambda^2 - \frac{1}{8}\lambda^4$, and consequently $\frac{5}{2}q - \frac{275}{112}q^2 = \lambda^2 - \frac{1}{8}\lambda^4$. But $\lambda = \frac{1}{300}$ nearly, therefore $\frac{\lambda^4}{4}$ is about $\frac{1}{32400000000}$, which may be safely neglected. And finally,

$$\lambda^2 = \frac{5}{2}q - \frac{275}{112}q^2 = \frac{5}{2}q\left(1 - \frac{55}{56}q\right) \quad .\ .\ .\ .\ (4)$$

As q is $\frac{1}{289 \cdot 4}$ nearly, $\frac{55}{56}q = 0 \cdot 0033936$: therefore $\lambda^2 = \frac{5}{2}q$ $(1 - 0 \cdot 003396) = 2 \cdot 491516\, q$ very nearly $= p$.

But Phil. Mag. vol. lxiv. page 163,

$$\varepsilon = p \times \frac{\varphi}{1+\varphi} - \frac{y}{z} = \frac{p\, r}{r + \left(\frac{t}{2}\right)^2 z} - \frac{y}{z} \quad .\ .\ .\ .\ (5)$$

And taking the radius of the equator and time of rotation as formerly stated, we have

$$\varepsilon = \frac{5212458}{20919576 + 1856062635\, z} - \frac{y}{z} \quad .\ .\ .\ .\ .\ (6)$$

From Captain Sabine's observations combined with the French in page 351 of his work, he obtains $l = 39 \cdot 01520 + 0 \cdot 20245 \sin^2 \lambda$; and adopting this, we can on the principles formerly laid down, (that is, rejecting those at St. Thomas and Ascension, as on basaltic bases,) deduce the length of the equatorial pendulum from observations at

Maranham . . . . = 39·01175
Sierra Leone . . . = 39·01556
Trinidad . . . . = 39·01193
Bahia . . . . = 39·01402
Mean . . . = 39·01332

On the same principles, from observations of the Spanish navigators as given in the *Connaissance des Tems* for 1816, we get, at

Acapulco	39·01126
Manilla	39·02067
Umatag	39·00157
Zamboanga	39·01379
Lima	39·01046
Isle Rabao	39·01370
Mean	39·01191

Also we have from several others, as

Formentera	39·01303
Madras	39·01275
San Blas	39·01311
Rio Janeiro	39·01447
Paramatta	39·01250
Mean	39·01254

As these means are tolerably consistent, we may group them all thus:

1. 39·01332
2. 39·01191
3. 39·01254

Mean of the whole, or $z =$ 39·01259 inches = 3·25105 feet. Substituting this value of in formula (6), it will become

$$\varepsilon = \frac{52127458}{20919576 + 6034152420} - \frac{y}{z}$$

or,

$$\varepsilon = 0\cdot008608 - \frac{y}{z} \;.\;.\;.\;.\;.\;.\;.\;.\quad (7)$$

Hence from Captain Sabine's book, page 351,

$$\varepsilon = 0\cdot008608 - \frac{\cdot20245}{39\cdot0152} = 0\cdot003419, \text{ or } \frac{1}{292\cdot5} \text{ instead of}$$

$\frac{1}{289}$ as he has found it.

He also gives the ellipticity for the lengths of several equatorial pendulums, page 352, such as 39·0152 and 39·01, and finds the difference inconsiderable. But when he changes the equatorial pendulum from 39·0152 to 39·01*, he retains the same total increase from gravitation, or 0·20245, with which we are by no means satisfied.

For if the equatorial pendulum be	39·01520
Total increase to the pole	0·20245
The polar pendulum would be	39·21765
Now if the polar pendulum remained the same and the equatorial became	39·01
the total increase would be	0·20765

These substituted in equation (7) would give an ellipticity of $\frac{1}{305}$, differing little from Laplace's estimate. The question, however, still remains,—are we at liberty to make such changes in either? It is at least unsafe. The better way would, in our opinion, be to determine the equatorial and polar pendulums

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* In fact, the ratios of ·20245 to 39·0152 and of ·20245 to 39·01 are nearly ratios of equality, as their value only differs about a unit in the sixth place of decimals! How then could there be any difference in the resulting compression?

lums by the means of a number of observations near those points; so that any small change or error of the total variation from the equator to the pole, when substituted in the formula may have little effect on their absolute lengths when reduced through a small arc by that formula. Thus at 70° N. Captain Sabine finds $39^{in}\cdot19452$ for the length of the pendulum by grouping those within 10° on each side of it. Now by reducing this to the pole, it becomes 39·21816, differing little from what we have already found it. The equatorial pendulum has, by nearly a similar manner, been found to be 39·01259, though Captain Sabine's group gives 39·01604. But if those at St. Thomas and Ascension he rejected as being on a basaltic basis, it will be 39·01308, differing little from the general mean of a number of places and different observers, and which we regard as the more decisive. Taking the polar pendulum at 39·21816, and the equatorial at 39·01259, the excess of the polar above the equatorial will be 0·20557, and hence the ellipticity will become $0\cdot008608 - 0\cdot005270 = \frac{1}{300}$ very nearly.

Hence, if we set out from the equator, the formula for the length of the pendulum at any latitude will be

$$l = 39\cdot01260 + 0\cdot20557 \sin^2 \lambda \quad \ldots\ldots \quad (A)$$

or, commencing at the parallel of 45° N.

$$l = 39\cdot11540 - 0\cdot102785 \cos 2\lambda \quad \ldots\ldots \quad (B)$$

From the foregoing results it appears that even if the quantities determined by Captain Sabine himself be substituted in the corrected formula for deducing the compression, it becomes considerably different from the fraction expressing the ratio of the centrifugal force to gravity at the equator, and still more so if the quantities which we have selected be adopted, as in our opinion best entitled to confidence.

By these remarks, it is by no means intended to set a light value upon Captain Sabine's labours. They will be highly estimated by all capable of appretiating their merit; but so much we are afraid cannot be said for the formulæ he has employed for deducing his final results. Approximations which might have been supposed sufficiently correct about a century ago, cannot now receive that appellation. What we have said is therefore rather intended to direct the attention of mathematicians to this subject, and to reconsider the degree of accuracy which may be conceded to the usual formulæ, than a critique on the labours of this distinguished officer, whose abilities and acquirements do so much honour to himself, and credit to his profession.

It is hoped Mr. Ivory, who is now examining with so much ability the *Mécanique Céleste*, will not allow this part of his subject to pass without receiving decided improvement at his hands.

Indeed the problem of the determination of the figure of the earth has now arrived at that point, (as Captain Sabine has remarked to me,) that no results from experiments on the pendulum should be admitted which are not of the accurate description; and I may add, that the requisite formulæ employed in making the usual deductions should also possess all the accuracy that the present state of science can give them.

I am, sir, yours, &c.

Edinburgh, Feb. 9, 1826. WILLIAM GALBRAITH.

XXV. *On the Number and Situation of the Magnetic Poles of the Earth. By Professor* CHRISTOPHER HANSTEEN.

[Concluded from p. 124.]

I. Observations of Declinations by Captain Sabine, in the voyage of Captain Ross in the year 1818. Extracted from the Philos. Trans. for the year 1819.

1818.	Lat.	Long. W. from Greenwich.	Declination. West.
	° ′	° ′	° ′
June 9	68 23	53 47	67 31
11—12	68 14	54 15	67 52
17—18	70 26	54 52	71 58*
27	71 2	54 13	75 30
July 4	72 44	56 49	78 55
6	73 22	57 32	80 1
12	74 1	57 52	80 44†
21	74 58	59 16	84 33
22	75 4	60 3	87 0
28	75 23	60 34	88 19
30	75 32	61 0	87 56
Aug. 2	75 45	64 0	88 57
4	75 59	64 32	90 18
6	70 51	64 34	91 8
12	75 55	65 30	93 40
19	76 30	72 35	102 36
22	76 33	76 53	107 56
25	76 9	78 21	109 58
Sept. 11	70 36	66 56	86 55

* Observations on Hare Island. † On the 3 Baffin's Islands.

II. Ob-

II. Observations of Declinations by Captain Parry. Extracted from Brewster's Philos. Journ. July 1821.

1819.		N. Lat.	Long. W. from Greenwich.	Declination.
		° ′	° ′	° ′
June	19	59 49	48 9	48 38 W.
	26	63 58	61 50	61 12
	27	63 44	61 59	60 20
	30	63 28	62 9	61 23
July	15	70 29	59 12	74 39
	17	72 0	59 56	80 55
	23	73 4	60 12	82 20
	24	73 0	60 9	81 34
	31	73 31	77 23	108 47*
Aug.	3	74 25	80 8	106 58
	7	72 45	89 41	118 16†
	13	73 11	89 23	114 17
	15	73 33	88 18	115 37
	22	74 40	91 47	128 58‡
	28	75 9	103 45	165 50§ E.
Sept.	1	75 3	105 55	158 4
	2	74 58	107 3	151 30
	6	74 47	110 34	126 17
	15	74 28	111 42	117 52
1820.		74 47	110 49	127 48‖
June	3	75 7	110 28	128 30
	7	75 35	110 36	135 4
	11	75 13	111 52	125 15
	12	75 5	111 57	123 48
	13	75 3	111 37	126 2
	15	74 49	111 12	123 6
Aug.	5	74 24	112 53	110 56
	10	74 26	113 48	106 7
	18	74 25	112 41	111 19
	25	74 27	112 11	114 35
Sept.	3	71 16	71 18	91 29 W.
	7	70 22	68 37	80 59

From these observations (which, for the purpose of avoiding the influence of the iron on board the vessels, were all made on shore, or on icebergs) it appears, that in Baffin's Bay and near 64° of longitude the declination already amounts to 90°, and thence increases westward; that Capt. Parry found it on the 22d of August 1819, in latitude 74° 40′ and longitude 91° 47′ = 128° 58′ W.; and on the 28th of August in latitude 75° 9′ and longitude 103° 45′, = 165° 50′ E. During the six days' interval (those in which no observations were made), the western declination must have risen to 180° before it became easterly, as it was found on the 28th of August. Dr. Brewster thence concludes, that between the 23d and 28th of August the expedition must have been several degrees north of the great magnetic pole; adding, that this circumstance fully agrees with the position given to it for that year in my investigation of the magnetism of the earth.

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* In Possession Bay. † East coast of Regent's Island.

‡ Cape Riley. § South-east point of Byam Martin's Island.

‖ In Winter Harbour on Melville's Island. All the observations from 2d September 1819 to 25th August 1821, were made on this island, for the most part during an excursion in the interior.

If in these investigations we also consider the dip of the needle, it is evident that above the pole the dipping needle must assume a vertical position,—that here therefore the dip is 90°; that the same must decrease the further we remove from this point, that it disappears entirely somewhere near the equator, and at last becomes southerly. Thus, for instance, the northern dip in Paris is = 68° 38′, in Copenhagen = 70° 37′, in Gothenburg = 72° 1′, in Christiania = 72° 45′, in Bergen = 74° 3′ &c. Therefore the observations on the dip may also be referred to, if we wish to ascertain the position of the magnetic pole of the earth.—The observations made on this subject by the two English north polar expeditions are contained in the two following tables:

I. Observations made during the voyage of Captain Ross.

1818.		North Lat.		Long. W. from Greenwich.		Dip.		
		°	′	°	′	°	′	
April	13	53	51	0	8	70	35	*
	30	60	9	1	12	74	21	†
June	9	68	22	53	50	83	8	
	19	70	26	54	52	82	49	‡
July	8	74	4	57	52	84	9	§
	23	75	5	60	3	84	25	
August	2	75	51	63	6	84	45	
	4	75	59	64	17	84	52	
	19	76	32	73	45	85	44	
	20	76	45	76	0	86	9	
	25	76	8	78	29	86	0	
Sept.	11	70	35	66	55	84	39	
Nov.	3	60	9	1	12	71	21	‖
1819. March		51	31	0	8	70	33	¶

* Regent's Park, London.

† Island of Brassa, Shetland.

‡ Hare Island.

§ On the three Baffin's Islands.

‖ Island of Brassa.

¶ Regent's Park, London.

II. Observations made by Captain Parry.

1819.	North Lat.	Long. W. from Greenwich.	Dip.
	° ′	° ′	° ′
March	51 31	0 8	70 33*
June 26	64 0	61 50	83 4
July 17	72 0	60 0	84 14
31	73 31	77 22	86 3†
August 7	72 45	89 41	88 27‡
11	72 57	89 30	88 25
15	73 33	88 18	87 36§
28	75 10	103 44	88 26‖
30	74 55	104 12	88 29
September 6	74 47	110 34	88 30 }¶
11	74 27	111 42	88 37
1820. July 18	74 47	110 48	88 43**
September 17	68 30	64 21	84 21
28	51 43	0 14	70 33††

From these observations it would appear, that the greatest dip was found by Captain Ross on the 20th of August 1818, near the entrance of Sir James Lancaster's Sound, into which he did not venture to penetrate; but that Captain Parry, after having proceeded up that sound, found a regular increase of the dip, till on the 11th of September 1819 it had risen to 88° 37′, leaving the needle only 1° 23′ from the vertical position. We may then conclude from this increase of the dip, that the expedition was about 3° north of the point where the dip is 90°, which also agrees pretty nearly with the point of convergence which we have deduced before from the observed declinations. Thus then, according to the indication of both instruments a magnetic pole exists in that vicinity.

If we consider the southern segment of the globe (Plate II.), we see that between the meridians 50° and 140° all the arrows are directed to one point, which is about 20° distant from the antarctic pole, and 137° east of Greenwich. To the east of the meridian of 140°, and to the west of that of 40°, the arrows begin to deviate from this point; and in the vicinity of Terra del Fuego, between 240° and 300° of longitude, they are again directed to another point, distant about 32° from the pole, and situated in 237° longitude. Thus the southern hemisphere has, like the northern, two different points of magnetic attraction. For the calculation of the position of the first, I have made use of the following observations:

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* Regent's Park, London.
† Possession Bay.
‡ East coast of Regent's Island.
§ North side of Barrow's Strait.
‖ Byam Martin's Island.
¶ Melville Island.
** Observatory in Winter Harbour.
†† Near London.

1773.	South Lat.	Long. E. from Greenwich.	Declination West.	No.
Cook.	° ′	° ′	° ′	
February 20	58 46	91 58	40 31	1
March 3	60 12	110 52	39 15	2
6	59 56	119 7	32 11	3
7—8	59 44	121 19	28 44	4
1777. Jan. 8	47 37	99 21	25 29	5
14	47 19	115 28	17 34	6
Fourneaux 1773.				
February 20	52 20	99 23	30 11	7
21	52 8	100 6	29 11	8
27	50 34	118 51	15 37	9
28	49 30	124 17	11 18	10

From these observations we find the position of the point of convergence thus:

From No.	Distance from the Pole.	Longitude East from Greenwich.
	° ′	° ′
2 and 4	20 26	138 7
1 — 4	19 46	140 0
2 — 10	20 58	135 12
2 — 9	21 30	132 47
7 — 10	19 47	136 31
8 — 10	19 53	136 25
2 — 3	20 27	138 29
5 — 6	19 39	138 11
9 — 10	18 12	138 36
3 — 9	21 48	134 21
Mean	20 14′·6	136 53′·4

If we reject the results of 1 and 4 and 9 and 10, which differ most from the others, we find

Distance from the pole = 20° 33′·5
Longitude E. from Greenwich = 136 15 ·4

The following are the observations from which I determined the point of convergence south of Terra del Fuego.

1774.		South Lat.	Long. E. from Greenwich.	Declination East.	No.
Cook.		° ′	° ′	° ′	
January	28	69 37	252 6	22 41	1
	29	70 20	253 3	24 39	2
December	13	53 24	270 30	13 23	3
	29	55 20	293 55	23 52	4
Fourneaux.					
January	24	59 37	256 2	12 59	5
	28	61 47	271 50	22 59	6
	29	61 53	276 45	24 1	7
	30	61 30	281 57	25 13	8
	31	61 20	288 10	26 6	9

The result of this is:

From No.	Distance from the Pole.	Long. E. from Greenwich.
	° ′	° ′
2 and 7	12 36	237 8
2 — 8	12 44	237 39
2 — 6	13 15	239 18
7 — 9	12 46	235 53
1 — 8	12 47	236 12
4 — 5	14 19	242 33
3 — 4	14 48	247 21
Mean	12 50	237 14

Omitting the two last, we find:

Distance from the pole = 12° 43′
E. longitude from Greenwich = 236 43

These two magnetic poles of the southern hemisphere also alter their position. From the observations made by Abel Jansen Tasman, who, proceeding from the Mauritius, discovered the islands of Van Diemen's Land and New Zealand, in the year 1643, I have determined the position of the magnetic point of convergence south of New Holland, for the year 1642, as follows:

Distance from the pole = 18° 55′
E. longitude from Greenwich = 146 59

The distance we found above for the year 1773 was

Distance from the pole = 20° 33′
E. longitude from Greenwich = 136 15

It is possible that the determinations for the year 1642 may not be quite correct, since at that period they had no means of giving the exact longitude; nevertheless I do not believe that the uncertainty with respect to the longitude of this point amounts to one degree. Thus then this magnetic pole has moved within 131 years, 10° 14′; or 4′·69 per annum westward. The situation of the other magnetic pole south of the continent, I have fixed for the year 1670, from some observations mentioned by Halley in his table of variations of the magnetic needle (Philos. Trans. No. 148), as follows:

Distance from the pole = 15° 53′
E. longitude from Greenwich = 265 26½

We have found the situation of this pole for the year 1774:

Distance from the pole = 12° 43′
E. longitude from Greenwich = 236 43

Thus this pole has moved within 104 years 28° 43′½, or 16′·57 annually, westward.

Whence we see that *the two magnetic poles in the northern hemisphere move eastward, while those in the southern hemisphere move westward.*

For the sake of abbreviation we will designate the south-eastern pole below New Holland, by A; the south-western below Terra del Fuego, by *a*; the north-westerly in America, by B; and the north-easterly in Siberia, by *b*. Thus A and B are very nearly diametrically opposed to each other: for the distance of both from the pole is about 20°; and A lies in the meridian 136°, and B in that of 260° E. of Greenwich, which makes a difference in longitude of about 125°. The case is similar, although with greater deviations, with the points *a* and *b*; the distance of the former from the antarctic pole being 13°, and of the latter from the arctic pole a little above 4°; and the longitude of the former being = 237°, and that of the latter 116°; thus giving a difference in longitude of = 121°. Experience, however, teaches us that there are no magnets with one or three poles, *i. e.* with any odd number of poles; a result which might have been found *à priori*, as the magnetic force only arises from a destruction of the equilibrium in the opposite powers; whilst one power prevails in one part of the body, the other must be forced into the opposite extremity. Therefore a magnet of several poles must be considered as an assemblage of magnets each of which has its two poles. Thence we must consider the four magnetic points found above, as the ends of two magnetic axes:—which of them belong to one another, can only be ascertained by a combination of the declinations and dips calculated from theory with those found by observation. That hypothesis must be correct, in which theory and experience agree. The poles A and B are at about the same distance from the terrestrial poles, and therefore very nearly diametrically opposed: besides, they are much stronger than the poles *a* and *b*; whence it seems natural to assume that A *and* B *are the terminating points of one magnetic axis, and* a *and* b *those of the other.* Therefore these two magnetic axes cross one another without intersecting each other, or passing through the centre of the earth: the centres of both lie much nearer the surface in the South Sea, than on our side of the earth.

This naturally produces several questions, which we cannot yet answer satisfactorily: *What is it that produces these two magnetic axes in the earth? What is the cause of their motion? How are we to imagine the possibility of this motion within the solid mass of the earth?* Concerning the first question, we have to consider that the magnetic powers are incorporeal essences, which, like the light, penetrate the densest bodies without being subject to the laws of gravitation. A magnetic axis, therefore, is nothing but a direction in a physical body in which these powers act. In a prismatic piece of steel these powers may be separated by simply passing a magnet over it; they may be destroyed by rubbing in a contrary direction; or they may even be inverted, so that a northern pole be changed into a southern, and *vice versâ*, without the internal position or mechanical connexion of the material particles being in the least altered. If then the interior mass of the earth consists of a material in which magnetic powers may be excited (and the above experience compels us to assume this hypothesis), the same causes which have excited the magnetic power may, under different circumstances, produce a different direction in the position of the axes, without there being any necessity of having recourse to an internal mechanical motion. Thus then the answer to the first question will probably also include that to the second. That to the third is of no difficulty. Light is the active principle of nature. The effect produced by the solar light, and the warmth excited by it, on the surface and atmosphere of the earth, is sufficiently known. The development and precipitation of aqueous vapour in the atmosphere, and the electricity which is thereby excited, are the effects of light and heat most generally known. The essential difference between electricity and magnetism, which was assumed according to former experiments, but in spite of philosophical misgivings has been removed by Œrsted's discovery. It is possible that the various illumination and heating of the earth during the period of one revolution on its axis, may produce a magnetic tension, as well as it produces the electrical powers, and that the altered position of the magnetic axes may be explained from an altered position of the terrestrial axis towards its orbit. Let it however be understood, that I advance these positions merely as suppositions.

It is proved then that the earth has two magnetic axes; and, consequently, four magnetic poles, of which the two northern turn from west to east, and the two southern from east to west, but with great difference in their motion. Let us see whether the variation in the declination may be explained from this. In the beginning of the 17th century the declination throughout all Europe was eastward; then it decreased, and disappeared a short time after the middle of that century; then became westward, increasing till within the late years, when it began to become invariable, and even to decrease.—Thus the declination in Paris was in

1541 =	7° 0′ E.	1667 =	0° 15′ W.
1550	8 0	1670	1 30
1580	11 30	1680	2 40
1603	8 45	1683	3 50
1630	4 30	1700	7 40
1640	3 0	1800	22 12
1659	2 0	1807	22 34
1664	0 40	1814	22 54
1666	0 0	1824	22 23¼ [*]

From the motions of the two northern magnetic poles found before, it appears that in the year 1580 the Siberian pole *b*, was about 40° E. of Greenwich, *i. e.* north of the White Sea; whilst the American B, was in about 224° E. from Greenwich, and thus somewhat above 30° east from Behring's Straits. Thus the former lay much nearer Europe than now; and the latter was further off:—thence the effect of the former was greater in Europe than that of the latter, and the needle turned towards the east. In the mean time the first removed towards the Siberian Ocean; and as the second approached Europe, although rather slowly, its effect became stronger, and the needle turned westward, till it has now attained its greatest declination, and will probably again approach the meridian. In the same manner it may be explained why the eastern declination was less before the year 1580.

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[*] I have added this declination from the *Annales de Chimie*, tom. xxvii. p. 436.—K.

The changes in the southern hemisphere may also be explained from the above-mentioned motions of the magnetic poles. Thus, for instance, at the Cape of Good Hope and in the different bays of the adjoining sea, the declination, during the time of Vasco de Gama was easterly (*i. e.* the northern pole of the needle pointed to the east, the southern to the west); but subsequently it became westerly, and that by more than 25°. It was in the year

1605 =	0° 30′ E.	1724 =	16° 27′ W.
1609	0 12 W.	1752	19 0
1614	1 30	1768	19 30
1667	7 15	1775	21 14
1675	8 30	1791	25 40
1702	12 50	1804	25 4

But in the year 1605 the position of the South American magnetic pole *a* was 283°½ E. *i. e.* nearly south of Terra del Fuego; and the New Holland magnetic pole A lay about 150° E. from Greenwich. The first point lay therefore much nearer the Cape of Good Hope, which is about 18° E. from Greenwich, than it does now, whilst the latter was further from it. Thence the effect of the former on the needle was stronger than at present, whilst that of the latter was weaker; and the southern pole of the needle moved more towards the west, and the northern more towards the east. But as the American southern point went further off, and that of New Holland approached, the southern pole of the needle turned more and more towards the latter, whereby the declination became westerly.

The dip also varies on different parts of the surface of the earth, increasing in some, and diminishing in others. Thus it was in Paris:

in 1671 =	71° 0′	1798 =	69° 26′
1754	72 15	1806	69 12
1780	71 48	1814	68 36

In eastern Siberia and at Kamtschatka it increases. Both are the results of the motion of the Siberian magnetic pole towards the east, in which it is removing from Europe, and approaching Kamtschatka. In the whole of South America the southern dip decreases, and that in consequence of the motion of the south-western magnetic pole, towards the west.

APPENDIX.

On the Noise attending the Aurora borealis[*]; *in a Letter from M.* RAMM, *Royal Inspector of Forests at Törset, to Professor Hansteen.*

I.

I have been much pleased with several Numbers of the *Magazin for Naturvidenskaberne*, but particularly so by your article on the magnetism of the earth. On reading Scoresby's voyage for the re-discovery of the east coast of Greenland, I thought to observe that neither he nor anybody else had noticed the noise attending the motions of the northern lights. I believe, however, that I have heard it repeatedly during a space of several hours, when a boy of ten or eleven years old (it was in the year 1766, 1767 or 1768); I was then crossing a meadow, near which was no forest, in winter, and saw for the first time the sky over me glowing with the most brilliant light playing in beautiful colours, in a manner I have never seen since. The colours showed themselves very distinctly on the plain, which was covered with snow or [illegible]-frost, and I heard several times a quick whispering sound simultaneously with the motion of the rays over my head. However clear this event is, and always has been, in my memory, it would be unjust to expect it to be received as an apodictical truth; but should others have made similar observations, it would be important for the inquirer into the nature of the *aurora borealis.*

Ramsmoen in Törset, March 1825.

Postscript to the above; by Professor HANSTEEN.

II.

I feel indebted to M. Ramm for the above communication. The polar regions are in reality the native country of the polar light; wherefore we ought to be peculiarly interested in obtaining any additional information on the natural history of this remarkable phænomenon; and we have so many certain accounts of the noise attending it, that the negative experience of southern nations cannot be brought in opposition to our positive knowledge. Unfortunately, we live, since the beginning of this century, in one of the great pauses of this phænomenon, so that the present generation knows but little of it from personal observation. It would therefore be very agreeable to the editors of the *Magazin*, to receive from older people similar experiences from the time of their youth, when the *aurora borealis* yet showed itself in its full splendour. It can be proved mathematically, that the rays of the northern lights ascend from the surface of the earth, in a direction inclining towards the south (an inclination which with us forms an angle of about 73°). If then this light occupies the whole northern sky, rising more than 17° above the zenith, the rays must proceed from under the feet of the observer, although they do not receive their reflecting power till they have reached a considerable elevation, perhaps beyond our atmosphere. It is therefore conceivable why we should frequently hear a noise attending the northern lights, when the *inhabitants of southern countries*, who see these phænomena at a distance of several hundred miles, hear no report whatever. Wargentin, in the 15th volume of the Trans. of the Swed. Acad., says that Dr. Gisler and Mr. Hellant, two scholars who had resided for some time in the north of Sweden, having been so requested by the Academy, made a report of their observations on the *aurora borealis.*

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[*] From the *Magazin for Naturvidenskaberne*, for the year 1825, part [illegible] p. 171—176, translated into German by L. F. Kæmtz.

The following is an extract from Dr. Gisler's account:

"The most remarkable circumstance attending the northern lights is, that although they seem to be very high in the air, perhaps higher than our common clouds, there are yet convincing proofs that they are connected with the atmosphere, and *often descend so low in it, that at times they seem to touch the earth itself, and on the highest mountains they produce an effect like a wind round the face of the traveller.*" He also says that he himself as well as other credible persons, "*had often heard the rushing of them, just as if a strong wind had been blowing* (*although there was a perfect calm at the time*), *or like the whizzing heard in the decomposition of certain bodies during a chemical process.*" It also seemed to him that he noticed "*a smell of smoke or burned salt.*" "I must yet add," says Gisler, "that people who had travelled in Norway, informed me they had sometimes been overtaken on the top of mountains by a thin fog, very similar to the northern lights, and which set the air in motion; they called it *Sildebleket* (Häring's lightning), and said that it was attended by a piercing cold, and rendered breathing difficult." Dr. Gisler also affirms that he heard repeatedly "*of a whitish gray cold fog with a greenish tint, which, although it did not prevent the mountains from being seen, yet somewhat obscured the sky*, rising from the earth, and changing itself at last into a northern light; at least such a fog was frequently the forerunner of this phænomenon."

Capt. Abrahamson, in the publications of the Scandinavian Literary Society, has also collected several observations of noises that were heard with the northern lights. I know myself several persons who have witnessed it, and shall make use of their observations on the first opportunity.

XXVI. *Con-*

XXVI. *Continuation of the New Catalogue of Meteorites. By* E. F. F. Chladni *.

I. *Additions to the Catalogue of Falls of Meteoric Stones and Masses of Iron.*

BESIDES the meteoric stones preserved at Abydos and Cassandria, as mentioned by Pliny, Hist. Nat. ii. 58, Joh. Laurentius Lydus, in his work *de Ostentis*, mentions after Apuleius (probably from some MS. of this author since lost), a similar stone preserved at Cyzicus, and of which it was thought that if it ever perished, the town would perish with it.

During the consulate of Cn. Calvinus and M. Messala, *i. e.* about fifty-two years before our æra, there were, according to Dio Cassius, xl. 47, falls of earth and stone.

During the consulate of Æmilius, 3 Kalend. April, a fall of stones took place at Vejae, according to an account taken from an ancient publication in the form of a newspaper (*Acta diurna, Acta urbis populique*), and communicated in the Roman newspaper called *Notizie del giorno* 1822. (As it is not said which Æmilius it was, it is impossible to point out the precise year.)

In the year 921, some stones fell near Narni, in the vicinity of Rome, which were considered infernal productions; one in particular which fell into the river Narnus, and which is said to have projected two feet above the surface of the water, according to a MS. chronicle by the friar Benedictus de St. Andrea, in the library of prince Chigi at Rome.

? 1201. Probably stones with a fiery meteor, according to a passage of Cardanus mentioned in *Lubienicii theatr. comet.* ii. p. 226, but which I have not been able to find in his own works.

Not long before 1349, in Arragon, three large stones, according to a MS. continuation of the Chronicle of Martinus Polonus, in the Hungarian National Museum at Pesth.

About the year 1780, in North America, in the territory of Kingsdale, in New-England, between West River Mountain and Connecticut, masses of iron.—Quarterly Review, No. 59. April 1824.

1818, 11th of June (or 30th of May, O. S.). Stones near Zaborzyca, in Vollhynia, according to Laugier, in the *Bulletin de la Soc. Philomat.* June 1823, p. 86; and *Mém. du Muséum*, 17 *Année*, t. xvi. *des Annales*, cah. 2.

? 1822, 10th of September. Probably meteoric stones, near Carlstadt in Sweden†.

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* From Schweigger's *Journal*, N. H. Band xiv. p. 475.

† We omit the accounts which here follow in the original, of the rece[illegible] falls of meteorites we have already incorporated with Dr. Chladni's former Catalogue;—see our present volume, p. 12.—Edit.

1824, about February. At some distance from Irkutsk, a large stone, according to newspaper accounts from St. Petersburg.

1824, 14th of October. Near Zebrak, in the circle of Beraun, in Bohemia, a stone, according to Hallaschka, in Schumacher's *Astron. Nachr.* No. 70.

? Small crystalline stones, or perhaps small pieces of magnetic pyrites, which (according to an account of Dr. Eversmann, communicated by Professor John, in the *Annalen der Physik*, v. lxxvi. p. 340; and in Kastner's *Archiv für Naturkunde*, v. iv. p. 2. p. 196) fell at Sterlitamansk in the government of Orenburg. It is however doubtful whether they belong to the class of meteoric stones, or whether they form a concretion of a peculiar kind.

(The pretended rain of stones mentioned in the newspapers as having fallen near Torresilla de Carneros in Spain, the 25th of July 1825, seems to have been nothing but common hail, of which pieces weighed as much as from 5 to 16 ounces.)

II. *Additions to the List of solid Masses of Iron containing Nickel.*

A. *Cellular Masses of solid Iron, with Portions of Olivine in the Interstices.*

The analyses of the olivine contained in such masses made by Counsellor Stromeyer are very remarkable, since they establish: 1. that the olivine of such masses contains no nickel; 2. but that, on the contrary, all other olivine and chrysolite contains nickel; 3. that olivine, chrysolite, and the species of stone of such solid masses of iron, belong to the same class*.

To these masses we have to add one found in the year 1809, in the vicinity of Brahin and Rzeczyca, in the government of Minsk, which seemed to have fallen a short time previously. Further particulars of it are given in the *Journal für Chemia*, &c. new series, vol. xiii. h. 1. p. 25†.

B. *Solid Masses of Iron containing Nickel.*

The mass found near Bitburg has been melted, through ignorance, and fragments of it have been again discovered by Professor Nœggerath: according to the analyses of Professor Bischof and Counsellor Karsten, they are of this kind‡.

Besides the masses of iron found in Louisiana near the Red River, and mentioned before, several more have been found, in the same vicinity, according to the Minerva, p. 1. vol. 1. n. 12, 26th of June 1824, published at New York.

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* See Phil. Mag. vol. lxvi. p. 357.

† Ibid. vol. lxv. p. 411.

‡ Ibid. vol. lxv. p. 401.

III. *Additions to the Catalogue of fallen Substances not being Meteoric Stones or solid Iron.*

1792, the 27th, 28th, and 29th of August. A rain of dust for three days without intermission, in the vicinity of La Paz in Peru, which could not have proceeded from a volcano. At the same time explosions were heard, and the sky was seen inflamed.—*Mercurio Peruano*, t. vi. 7th December 1792.

1824, 23d of August. At Mendoza in South America, near the river Plate, from a black cloud, a rain of dust with which the whole city was covered. Forty miles from the city the same cloud discharged itself again.—From a Buenos Ayres newspaper, 1st November 1824.

1824, 17th of December. About a quarter after six o'clock in the evening, at Neuhaus in Bohemia, a bituminous mass must have fallen, accompanied by a globe of fire (a phænomenon which has frequently happened before) which had been seen to descend there, since a part of the meteor remained burning against the church steeple for a quarter of an hour.—*Haude und Spenersche Zeitung* of Berlin, No. 7. 10th of January 1825*.

On the Mechanical Structure of Meteoric Stones.

Dr. G. Rose of Berlin has succeeded in separating crystals of pyroxene from a large fragment of the meteoric stone of Juvenas, the angles of which he has measured with a reflective goniometer. One of these crystals is that modification of the octahedron which is represented in Haüy's Mineralogy, fig. 109. The same structure also includes microscopic macled-crystals, which seem to be Labrador-spar. [?]

At the request of M. Humboldt, Dr. Rose has also examined the meteoric mass of Pallas, and the trachytes of Chimboraço, and other volcanos of the Andes. He found the olivine of the Siberian mass perfectly crystalline; the trachytes contained for the most part inclosed crystals of albite and hornblende.

This notice may serve as an explanation of the imperfect account given before.

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* I ought also to mention that we have lately had an analysis by M. Buchner, in Kastner's *Archiv.* vol. v. p. 182, of a slimy meteor. It was found to be an organic substance like a mucus, containing mephitic matter.

XXVII. *Some*

XXVII. *Some Account of the Dissection of a* Simia Satyrus, *Ourang Outang, or Wild Man of the Woods. By* John Jeffries, *M.D.*[*]

THE attention of the medical profession to comparative anatomy, and the interest which the naturalist feels in prosecuting this interesting study, are my inducements for offering the following account of an animal which forms, in the chain of created beings, the connecting link of brutes to man.

Many have been disposed to doubt the existence of such an animal as the *Satyrus*, and more have been incredulous of any remarkable similarity in structure to man. Such doubts, I think, must be removed, by an examination of the anatomical preparations I have been enabled to make of him[†].

This animal is a native of Borneo, an Asiatic island under the equator, in longitude from 110° to 120° east. This individual was carried from Borneo to Batavia, and came into the possession of Mr. Forrestier of that place, where he remained some time. By him he was sent, consigned to Mr. Charles Thatcher, merchant, of this place, in the Octavia, Captain Blanchard. He died on the night of the second of June, the first after his arrival; disappointing the sanguine expectations of his owners of great pecuniary remuneration from his exhibition in public.

In his external appearance, our subject resembled an African, with the neck somewhat shorter and the head projecting more forward. He was three feet and a half in height. He was covered with hair, except his face, the palms of the hands and feet, which were all of the colour of the negro. The hair was of a dun colour, inclining to black. It resembled the hair of the human body more than that of brutes, in consisting all of one kind, and not, as in quadrupeds, of two forms of plicæ. On the head the course of the hair was forward and upward; before the ears it was downwards. There was very little on the anterior part of the head, leaving him an extensive forehead. On the arm, its course was down; on the fore arm, up. It was longest on the back of the arms and thighs, measuring from six to seven inches. His ears were thin, small, and handsome, lying close upon the head

His eyes were hazel-coloured, bright, and somewhat deep in the sockets. His brow was prominent, to defend the eyes from injury in the woods. He had very little hair on the brow. His nose was flat. His lips were very large and thick, more so than those of any negro I ever saw. His chin was broad and projecting, as was likewise the upper jaw. His chest was round, full, and prominent. His shoulders were set well back. His scapulæ were flat and close behind. His waist was small. His hips were flat and narrow. His arms were very long, the fingers reaching to the ancles. His lower extremities were short and small in proportion to the rest of the animal. He had the spiral lines like human, on the tips of the fingers, and the lines of palmistry on the hands, and also on the lower limbs. He had the bend of the spine above the sacrum. There was no projection of the coccyx. His nates were small, as were also the calves of his legs, which had however some figure. His mammæ and umbilicus were distinct. The scrotum was very small, being merely a little laxity of the skin at this part.

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[*] From Webster & Treadwell's Boston Journal of Philosophy, vol. ii. p. 570.

[†] I cannot sufficiently regret the season of the year in which he fell into my hands, which has prevented that patient and slow dissection which alone could enable me to give a correct and full description of his internal structure. The above is the best account I can offer, from a dissection prosecuted with the temperature for several days from 88° to 94°.

The account which I have learned from Captain Blanchard illustrates his habits and manners.

He was put on board the Octavia, under the care of this gentleman, and had a house fitted for him, and was provided with poultry and rice sufficient for the voyage. Captain Blanchard first saw him at Mr. Forrestier's house in Batavia.

While sitting at breakfast, he heard some one enter a door behind, and found a hand placed familiarly on his shoulder: on turning round, he was not a little surprised to find a hairy negro making such an unceremonious acquaintance.

George, by which name he passed, seated himself at table by direction of Mr. Forrestier, and after partaking of coffee, &c. was dismissed. He kept his house on ship-board clean, and at all times in good order; he cleared it out daily of remnants of food, &c. and frequently washed it, being provided with water and a cloth for the purpose. He was very cleanly in his person and habits, washing his hands and face regularly, and in the same manner as a man. He was docile and obedient, fond of play and amusement; but would sometimes become so rough, although in good temper, as to require correction from Captain Blanchard, on which occasions he would lie down and cry very much in the voice of a child, appearing sorry for having given offence. His food was rice paddy in general, but he would, and did, eat almost any thing provided for him. The paddy he sometimes ate with molasses, and sometimes without. Tea, coffee, fruit, &c. he was fond of, and was in the habit of coming to the table at dinner, to partake of wine; this was in general claret.

His mode of sitting was on an elevated seat, and not on the floor. He was free from some of the peculiar propensities of monkeys. His bowels were in general regular. The directions given by Mr. Forrestier were, in case of sickness, to give him castor oil. It was administered to him once on the beginning of the passage, and produced full vomiting and free catharsis with effectual relief. He sickened a second time on the latter part of the voyage, and resisted the attempts of the captain and several strong men to get the oil into the stomach. He continued to fail gradually, losing his appetite and strength until he died, much emaciated, soon after the ship anchored. Obstruction of the bowels was no doubt the source of his sickness and cause of his death. Captain Blanchard used to feel his pulse at the radial artery, and describes it to be like the human. It was probably quicker. His mode of walking was always erect, unless when tired; he would then move or rest on all fours*.

The skin was attached very closely to the body at all parts, particularly [illegible] the face, hands, elbows, and soles of the feet. He had no cutaneous muscles except the platisma myoides. This was not connected on its inner surface, but formed a large pouch extending from the chin to the sternum, continuing round to the sides of the neck. It was supposed by those who saw him, to be a receptacle for food. This was not however its purpose; for it communicated with the larynx and not with the pharynx, as will be described when speaking of those parts.

The abdomen presented a view so similar to the human, that it required some attention to note any peculiarities. The omentum was small, lying high up the intestines, coloured with bile, as were the bowels generally. The peritoneal folds were very strong, particularly the ligaments of the liver, the mesentery, &c.; the caput coli was also strongly confined to its place. The spermatic cord received its parts, and passed obliquely under the muscles and came out at Poupart's ligament, as in man. The proportion of the small to the large intestines was about the same as in the human subject. The arch and sigmoid flexure of the colon exceedingly resembled the human. He had the appendix vermiformis very long, measuring four inches. This I found full of small stones and some pieces of egg shell, together with liquid fæces. The large intestines were found loaded with indurated fæces from the caput coli to the extremity of the rectum. The stomach, in situation and figure, was like a man's; its cardiac orifice was perhaps smaller, and the pylorus larger; its dimensions were, when inflated, from one orifice to the other round the fundus, ten and a half inches; across it measured three inches. It was nine inches in circumference round the fundus. The spleen was attached by the vasa brevia, and in colour, size, and situation, accorded to man's. The liver was very much like ours, of a deep red colour, and divided into two lobes, but the fissure was not quite so distinct. In connexion with the other viscera, it appeared exceedingly like the human. The gall-bladder was much longer and smaller round, and was found full of dark inspissated bile, which could with difficulty be crowded along the duct. The pancreas laid upon the spine as in man. These had all their orifices opening into the bowels in the same way as the human. The kidneys did not present any difference, except that the renal capsules were larger. The bladder was small, containing when full, about two gills. The urethra, prostate gland, vesiculæ, &c. were situated like the human. The prepuce, glans, &c. were like the human, but small. The organs of the chest resembled the human in size, figure and situation. The lungs did not present quite so much difference on the two sides as in man; that of the left being nearly of the same size with the right, carrying the heart more towards the centre of the thorax. The lungs were not so distinctly divided into lobes; they were very sound and healthy in appearance. The heart was conical like man's, and in every respect resembled the human. The arch of the aorta and the descending aorta were small, in proportion to the size of the heart. The right subclavian, right and left carotid arteries, all arose from the arteria innominata; the left subclavian rose separately, near the base of this. The pericardium was connected extensively to the diaphragm, which was very large and strong. The chest was divided by the mediastinum, and the thymus gland laid between its sides.

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* This circumstance I was not informed of until after I had completed his dissection, and made observations, which close this communication. It did not therefore influence me in judging from his anatomical structure of his natural mode of walking.

The mouth and fauces resembled the human, except in dimensions; being much longer from front to rear. The velum palati was without the uvula, but broader and more lax. The body which answered the purpose of the uvula was situated on the posterior surface of the velum; and when this was forced backwards, exactly closed the posterior entrance of the nose. The glottis and epiglottis resembled the human. The os hyoides and cartilages of the larynx were much as in man. Between the os hyoides and the thyroid cartilage, there were on each side two openings about a quarter of an inch in diameter, leading into the larynx and coming out at the base of this cartilage. A valve played at the inferior opening, preventing the passage of an instrument downward, but it passed easily upwards into the pouch on the neck, which has been mentioned. This pouch, the animal could inflate at pleasure; for what purpose I do not know. One use might be, when inflated, to assist in supporting him when swimming.

The brain weighed nine ounces and three quarters. The nerves arose from this in the same manner as the human, and took their exit from the cranium in a similar way. The position of the brain differed by the anterior lobes being more raised in consequence of the projecting plates of the orbits internally, and by the posterior lobes and cerebellum lying lower than the human, according to the form of the base of the cranium. This organ was not dissected. The muscles and blood-vessels could not be so minutely examined, in consequence of the warmth of the season, as to enable me to give a correct account of them. The muscles were in general very distinct, having their fasciculi of fibres remarkably strong. The blood-vessels were small.

Description of the Skeleton.

The whole skeleton is three feet four inches high. From the first vertebra of the neck to the end of the coccyx, it measures nineteen inches.

From the head of the humerus to the end of the middle finger is thirty-one inches; the end of this finger reaches to the end of the fibula.

From the top of the trochanter major, to the bottom of the os calcis, is seventeen inches. The length of the foot is nine inches and a half. The length of the hand is eight inches.

A line drawn from the nose to the occipital protuberance, measures eight and a half inches.

Round the cranium over the orbits to the occipital protuberance is fourteen inches.

From the meatus auditorius of one side to that of the other, over the coronary suture, is eight inches.

The longitudinal diameter is four inches and an eighth.

The lateral diameter is three inches and a half.

The depth from the vertex to the foramen magnum is three and a quarter inches.

The sutures are serrated, and resemble very much the human.

It has the os triquetrum perfect.

The orbitar ridges are very prominent.

The styloid and mastoid processes are short.

The nasal bones are wanting, giving him that flat or simous appearance, from which is derived the term *simia*, to distinguish the ape species.

The maxilla superior and inferior are very prominent, which makes the facial angle more obtuse than the African.

The frontis is somewhat high and projected.

The inferior maxilla is closed at the mentum, which is a little angular and projecting.

The teeth consist in each jaw of two dentes incisivi; the two middle of the upper jaw are very long and broad, measuring seven-eighths of an inch in length, and five-eighths in breadth. The two lateral have not yet fully grown; two cuspidati, and four molar teeth; making in all, twenty-eight.

The four incisores are new and permanent teeth; the cuspidati had not been shed.

The first molar in each jaw was just giving place to the bicuspid.

The last molar in each jaw are permanent teeth, the others were about being shed.

I should judge from the teeth, that this individual was about five and a half years old.

The spine consists of twenty-three vertebræ, viz. seven of the neck, twelve of the back, and four of the loins.

The neck is short, being but three and a quarter inches in length. The vertebræ composing it are flatter before, and not so round, having their spinous processes much longer and rounder than in man.

The first vertebra of the neck has no spinous process, being in this respect unlike the human, which has a small one; but anteriorly, it resembles man, and differs from the monkey, in having an eminence rather than a fissure.

The second vertebra has the processus dentatus long, and partly cartilaginous; the transverse processes are so also.

The vertebræ of the back are like those of man; they measure eight inches and three quarters.

The vertebræ of the loins are three inches in length. They have their transverse and spinous processes short and thick, like man's.

The ilea are very flat, and are articulated to the sacrum as in man. The sacrum differs materially from the human, being more flat and narrow; it consists of five bones, connected by cartilage. Indeed, the whole pelvis exhibits a more striking difference from the human than any other part of the skeleton.

The ileum measures from the anterior superior spinous process to its junction with the sacrum, three inches.

The ilea, ischia, and pubes, are distinct bones, connected by cartilage. The symphysis pubis is also cartilaginous.

The lateral diameter of the pelvis is two and a half inches. The longitudinal diameter is three and a quarter inches.

The pelvis is so joined to the spine as to project backward, and so flat, that a perpendicular line from the bodies of the dorsal vertebræ falls upon the pubis.

The coccyx is cartilaginous, and resembles the human; it is not so long, and has no appearance of a tail.

The ribs are twelve in number, articulated and curved as much as the human, giving the animal a full chest.

There were eight true ribs attached to the sternum by cartilage, as in man, and four floating ribs.

The sternum consists of four bones like man's, but more cartilaginous; the ensiformis longer.

The clavicle remarkably resembles the human; it is not quite so much bent, and measures five and a half inches.

The scapulæ likewise resemble those of man; the base is narrower and longer; the acromion and coracoid processes are more cartilaginous than those of a child.

The chest gives the animal the greatest resemblance to man; the position of the shoulders, the articulations of the humerus, clavicle, and scapula, the angle of the ribs, the prominent thorax, the situation of the arms, all so much resemble the human, that they might easily pass for such.

The length of the humerus is eleven and a quarter inches; the head of the bone is cartilaginous; it is articulated like the human; on the lower part it is thinner and flatter than man's; the condyles are prominent and cartilaginous; the radius is eleven inches in length; it is somewhat curved anteriorly, in other respects it resembles the human.

The ulna is eleven and a half inches long; it has a large curved projection at the lower part for the insertion of muscles.

The bones of the carpus are eight in number, and resemble the human, except that they are all longer and a little narrower, more cartilaginous, and admit of more free motion upon one another.

The bones of the metacarpus are five in number, each about three inches in length, except that of the thumb, which is an inch and three quarters.

The thumb has two bones, and is an inch and a half in length.

The phalanx of the fore finger is four inches long, that of the middle and ring fingers, four and a half inches; the little finger is three and a half inches.

The articulation of the femur with the acetabulum is almost exactly like man's; the neck of this bone forms about the same angle. In quadrupeds, this forms a distinguishing characteristic, being in them nearly a right angle; the inspection of this joint is alone sufficient to satisfy the naturalist of at least the facility, if not the natural disposition of the *Satyrus* to walk erect.

The femur is eight and a half inches in length and two inches round; the trochanters and condyles are cartilaginous and prominent.

The patella is one round piece like that of man; it is but little ossified in this individual; the tendon connecting it to the tibia is strong. The knee joint has the semilunar cartilages and is connected by the crucial and lateral ligaments as in man. The tibia is seven and three quarters inches long, and two inches round at the upper part.

The fibula is seven and a half inches long, and an inch and a quarter round; the extremities of both bones of the leg are cartilaginous.

The ankle joint is formed like man's.

The tarsus consists of seven bones like the human; these are mostly cartilaginous, and admit of free motion one upon another.

The os calcis is broad, and sufficiently projects behind to support the erect posture. The metatarsus consists of four bones; for what answers to the great toe is a perfect thumb of two joints, but not on the range of the other toes; indeed the whole foot, except the os calcis, much more resembles a hand than a human foot, the phalanges being longer and consisting of bones similar to the hand.

From the structure which has been thus cursorily described, I shall note those peculiarities which will enable us to form an opinion of the natural mode of his walking.

First. Going on all fours, he would find inconvenience from the elbow joint; for when the hand is placed upon the ground flat, the flexion of the joint would be contrary to that of quadrupeds, by bending back towards the body instead of forwards, which would rather impede, than assist progression. It is not however as difficult for the *Satyrus* to turn the joint forwards, as it would be for man, on account of the curvature of the bones of the fore arm, and the free motion which existed in all the joints.

The roundness of the chest, and the scapulæ setting so far back, would make it difficult for him to bear weight upon his hands; quadrupeds have the chest flat and the scapulæ far forward upon the ribs.

The articulation of the hip would make it more easy for him to go erect, on account of the little angle made by the neck with the body of the femur.

Secondly. In walking erect, he would derive advantage from the extension of the os calcis and the length of the foot; and also from the position of the arms so far back, and from their length, which would enable him to balance the body by them.

Thirdly. From the structure of the viscera he seems to be peculiarly formed for an erect posture.

The pericardium being united extensively with the diaphragm, would prevent it from being drawn down by the weight of the liver and abdominal viscera. In quadrupeds this is not necessary, for the pressure of the abdominal contents assists expiration, and if the pericardium was attached to the diaphragm as in the *Satyrus* and in man, inspiration would be impeded.

The exit of the spermatic cord is another difference from quadrupeds. It does not pass out directly from the abdomen, as in the dog, but perforates the peritoneum and muscles obliquely, as has been described, thereby giving that admirable structure to fortify the groin from rupture, which exists in man.

The viscera of the abdomen were suspended to bear weight in the erect posture, particularly the liver, which had its ligaments very strong.

From these and other circumstances, apparent from an examination of the skeleton, I think we must conclude the erect posture to have been most natural. At least, if it is humiliating to dignify him with the title of a biped, he stands acquitted from that of a quadruped from the peculiar formation of his lower extremities. We must then denominate him, as some naturalists have done, a quadrumanus animal.

Note.—The preparations which have been made from him are

The skin stuffed and prepared to exhibit his external appearance.

His natural skeleton entire.

The heart fully injected, with the aorta and other vessels, and the lungs *in situ*, with a portion of the diaphragm.

The tongue, larynx, pharynx, &c. exhibiting the peculiar structure of its connexion with the pouch, and its general resemblance to man's.

Dried preparations of the stomach, caput coli, and its appendix, and of the urinary and gall bladders.

Boston, July 1, 1825.

XXVIII. *Description of a nondescript Species of the Genus* Condylura. *By* T. W. Harris, *M.D.*[*]

THE genus *Condylura* was constructed by Illiger for the reception of the *Sorex cristatus* of Linnæus, the *Radiated Mole* of Pennant.

This name, derived from κονδυλος a knot, and ουϱη the tail, is essentially bad, as it is founded on an exaggerated or caricatured representation of the tail of the animal, and on a structure which does not exist, in the slightest degree, in the species to be here described. Desmarest, who has amended the characters of the genus, did not think it expedient to change the name, and thus embarrass nomenclature with a new synonym.

Cuvier, in the *Regne Animal*, has suppressed the genus *Condylura*, being confident, he says, from an inspection of the teeth, that the radiated mole is a *Talpa* and not a *Sorex*. Desmarest[†] thinks that Cuvier must have examined, by mistake, the denuded head of a true *Talpa*, instead of that of the *Condylura*. He observes that a specimen of this animal, sent by Le Seuer from Philadelphia, presents characters peculiar to itself; that it cannot be united either with the *Talpæ* or *Sorices*, but holds an intermediate rank between these two tribes or families. In its form and habits it has an affinity to the former, while its teeth closely resemble those of the latter. It is arranged in the family *Soricii* and genus *Scalops* by the author of the article MAZOLOGY, in Brewster's Encyclopædia.

The *Sorex cristatus*, with another animal of the same genus recently detected in Maine, might with propriety constitute a new family with the following characters.

Upper and lower jaw each with twenty teeth; four incisors only in the lower jaw; nostrils carunculated; tail scaly, of moderate length; feet with five claws, the anterior ones broad, and formed for digging in the earth; the hind feet elongated, slender; eyes minute; and no external ears[‡].

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[*] From Webster & Treadwell's Boston Journal of Philosophy, vol. ii. p. 580.

[†] See article TAUPE; *Nouveau Dictionnaire d'Histoire Naturelle*, tom. xxxii. Paris, 1819.

[‡] The essential characters of the Shrew-mice or *Sorices*, are six or eight cutting teeth in each jaw, the intermediate ones the longest; tail and external ears sometimes wanting.

The family of the Moles, or *Talpæ*, is characterized by having twenty-two teeth in each jaw; six incisors in the upper and eight in the lower jaw, equal to each other; no external ears; tail very short; eyes and feet as in the *Condyluræ*.

The animals of this family, like the moles and shrew-mice, burrow in the ground, and live upon insects.

In March 1825, a small animal was discovered, near Machias, in the state of Maine, which exhibits the characteristics of the genus *Condylura*, but which is evidently distinct from *C. cristata*, the type of that genus. These animals both have in the upper jaw six incisors implanted in the præmaxillary bone, the two intermediate ones large, their cutting edge oblique; the adjoining incisors resembling long canine teeth, slightly triangular at the base, where are situated two minute tubercles; each external incisor isolated, very small, conic, and pointing backwards. Seven molares on each side; the three first resembling canine teeth, and may be considered as false molares; they are smaller than the true molares, are isolated with two minute lobes at the base. The four posterior molares large, formed of two layers of enamel, furrowed externally, and tuberculated within.

The palate has seven transverse ridges between the incisors and the first two molares.

Lower jaw with four flattened and projecting incisors; five false molares, separated from each other, the first the largest, and each of them with three or four small lobes; three true molares, composed of two layers of enamel, channelled within, and tuberculated on the outside.

Proboscis elongate, extensile; the nasal extremity naked, and bordered with about twenty cartilaginous, acuminated processes, disposed in a circle, the two superior ones united at the base, longer than the others, and situated a little in advance of them.

Neck indistinct; legs short, the hind ones placed far back; feet five-toed, the anterior ones very broad and scaly, with a series of curved hairs on the external edge; the nails long and straight. The hind feet a third longer than the fore feet, scaly, narrow, with a warty excrescence on the inner part of the tarsus; nails slightly curved and short. Tail scaly, and thinly covered with coarse hairs. Eyes minute. No external ears.

The species from Maine appears to be a nondescript, and may therefore receive the name of *prasinata*. It is clothed with a long and very fine fur of a *green* colour, with a few gray hairs at the extremity of the tail. The nose is naked, the caruncles, which surround it in a stellate manner, are twenty-two in number, and of a brownish hue. The eyes are exceedingly minute, and are entirely concealed by the fur. The fore feet greatly resemble hands; the palms are covered with a thick cuticle, and on the inside of each of the fingers, near their origin, are three triangular acuminated scales, or cuticular processes. A large rounded warty excrescence is situated midway, on the inner and lower part of the foot. The specimen was a male. The tail nearly three quarters the length of the body, very small, or strangulated at its insertion, becoming abruptly very large, and gradually tapering towards the extremity. The caudal vertebræ were not distinguishable through the mass of fat with which they were enveloped, and of which the tail was principally composed. There were no transverse folds or ridges on the tail, its surface being perfectly uniform, nor were the hairs disposed in distinct whorls. The tail of this species therefore differs essentially from that of the *cristata*, as described by authors, and induces us to wish that Desmarest had changed the name of the genus for some one more expressive of the species which compose it.

Length of the male *Condylura prasinata*, from the end of the snout to the origin of the tail, four and a half inches. Length of the tail three inches. Circumference of the body three inches and three quarters. Circumference of the tail, at the largest part, one and a half inch. Average length of the nasal radii five-twentieths of an inch. Length of the hand eight-tenths of an inch. Length of the longest nail three-tenths of an inch. Length of the foot one-inch and one-tenth. Length of the longest nail of the foot five-twentieths of an inch. Distance between the eyes rather over three-tenths of an inch. From the end of the snout to the eyes seven-tenths of an inch.

Milton, May 4, 1825.

XXIX. *On the Volcanic Origin of the Rock-salt Formation.*

By Dr. J. Nœggerath*.

I HAVE read with pleasure M. de Charpentier's letter of the 22d of March 1825, to M. L. de Buch, with the valuable remarks of the latter meritorious naturalist attached to it, which have appeared in Poggendorf's *Annalen der Phys. und Chemie* 1825, St. 1.

It describes a great vein at Bex in Switzerland, which is between perpendicular strata of anhydrite, and rises from 30 to 40 feet, with fragments of anhydrite: this vein is filled with silicate of lime, and a considerable quantity of sand and dust of anhydrite, which are collectively combined together into a firm mass by a pure rock-salt, perfectly free from water: this mass is covered with a powder and has no cavities with crystals. All this indisputably evinces, that it is a fissure

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* From Schweigger's *Journal*, N. S. Band xiv. p. 278.

produced

produced by volcanic power, into which the chloride of sodium has entered by sublimation. L. de Buch proves this in a highly convincing manner. Guided by that fact, and supported by other grounds, he further supposes that the rock-salt formations in the flœtz strata very probably have also a volcanic origin.

But I had already ventured to propose the same theory of rock-salt formations before the vein at Bex was known. The merit of this I must indeed acknowledge is not great: having the advantage of L. de Buch's work, it was easy for me to advance one step further than he had gone; or perhaps, I only expressed more definitely what L. de B. had long conceived, and was a simple consequence of his comprehensive observations. But since my theory has the concurrence of so valuable an experience as that of M. de Charpentier, it will not be wholly uninteresting to make my early expositions better known.

In the collection of foreign works published by me and M. D. Pauls on volcanos and the phænomena allied to them, the second volume of which (containing the volcanos at Java, by Sir T. S. Raffles; the Monte-somma by L. A. Necker, and the volcanos in Auvergne, by Dr. Daubeny) had already been printed at the end of February 1825, I put in a note a German translation of Humboldt's treatise concerning the appearance of sulphur in the primary rocks, according to Gay-Lussac and Arago, (*Annales de Chimie et de Phys.* 1824, Oct.) and I added the following remarks of my own.

" The excellent communications of A. von Humboldt afford us not only decisive proofs of the existence of sulphur in the primary rocks, but render it very probable also that they contain great masses of it. To ascertain the origin of the sulphur and its combinations in the fixed, fluid, and gaseous products of volcanos has hence lost the greater part of its difficulties. If on the one hand collective experience, and the theories that have been most recently raised on it, tend to establish Von Humboldt's remark (*Ueber der Bau und the Wirkungen der Vulkane*), that ' the powers of volcanos operate simultaneously, not superficially from the outer crust of the earth, but profoundly from the interior of our planet, through caverns and vacant passages, on its remotest points,' the existence of sulphur in the newer rocks, and especially in those that are formed in horizontal strata, cannot account for the important part which this mineral (sulphur) performs in volcanos. Von Przystanowski (*Ueber den Ursprung der Vulkane in Italie* 1822) has indeed the merit of having indicated two great tracts in Italy, in which the sulphur (with iron-pyrites, sulphuret of antimony, asphaltum, anthracite, and rock-salt) is diffused in the limestone, marl, and gypsum; and in regard to this fact, nothing more is requisite than to ascertain more exactly the relative ages of these formations.

" But if Von P. assumes that this sulphur is the cause of the existence and duration of the volcanos of Italy, we are by no means disposed to adopt his view; although he has very clearly shown that the three active volcanos of Italy, (Vesuvius, Stromboli, and Ætna); and moreover, the points which, according to history, have had only one vent (Ischia and the Monte-nuovo at Pozzuoli); and finally, those places of the Roman territory which display in the character of their mountains, traces of lava, and other effects of fire, as Valentano, Viterbo, Frescati, and perhaps Monte Rossi,—lie collectively in a flœtz district which he has indicated (part of it lying at Solfaterra), and which contain sulphur and other inflammable substances. It is certainly not denied that in this tract a chemical agency displays itself in various ways; but it still seems to us very different from the proper volcanic agency. We dare not assume with Von P. that the former can rise to the height or vehemence of the latter. If it also is the fact that we cannot always distinguish on the surface of the earth, effects whose causes are deeply hid in the bowels of our planet, from those which are peculiar to the newer formations of the earth's crust; and if, moreover, the earlier results of the chemical agency still operate in some places, it would be impossible to pass from the one class to the other, on account of the similarity between the weaker volcanic effects and the most striking phænomena, which are both produced on the surface of the earth by the same chemical process.

" Was then the coincidence of the locality of the Italian volcanos with the existence of the sulphuriferous tract merely accidental? That is a question which one might not answer exactly with a negative. Although the rationale of the association of those phænomena is not quite clearly before us, and we can only premise obscurely, only hint at what it is; still it is not impossible that time may completely prove it to us. Perhaps too what Von P. regards as a *cause* of volcanic powers, may be only an *effect* of them? Such a hypothesis can be viewed only as a geological heresy, since the illustrious L. de Buch has given a very credible explanation of former volcanic agency to so wide an extent. The proportionately limited presence of gypsum of all forms, and its accompaniments of rock-salt and sulphur in the various formations of limestone, had long ago been observed, and may indicate that the formation is in a state of decline. With a view to this question we venture to draw attention, particularly, to the interesting letter of L. de B. to M. Freisleben, concerning the Hartz.

"The presence of rock-salt and of muriatic acid in volcanic productions of every kind has appeared hitherto less strange than the presence of sulphur, because the sea-water which is supposed to flow to the foot of the volcanos, and to occasion their activity, may explain it. But if the metals and metalloids in the bowels of the earth are to be considered only as in the state of chlorides, as Gay-Lussac has rendered very probable *, the explanation would have still fewer difficulties.

"The local limitation and the concurrence of gypsum and rock-salt in rock-formations of the changeable and secondary kind, is a phænomenon too striking not to lead our minds necessarily to revert to them both, when we treat of the origin of the former. L. de Buch certainly has never made a remark to this extent in his essays; for he seems not to have made any general application even of his own theory of the formation of gypsum: as he only says, that it is *frequently* converted to limestone by the operation of internal causes upon it. But are not the products of the salt-formation actually produced from the salt-clays? We certainly are very well aware that the admission of the volcanic origin of rock-salt either by immediate or secondary agency, has still many difficulties, and we therefore readily value the idea only as a gentle hint, such as may very well be tolerated in the province of geology, which has not yet advanced beyond the age of fiction and hypothesis. At least this idea is not wholly without foundation; and we shall not mourn over its fall, if more particular experience should at some future time supplant it, or more correct conclusions be drawn from our present experience."

Four months ago I wrote this. Now, I should suggest the hypothesis still more boldly; for it has acquired important evidence, and its permanent confirmation has been rendered still more probable.

XXX. *On the Fossil Elk of Ireland. By* THOMAS WEAVER, *Esq. M.R.I.A. F.G.S. &c.*†

NOTWITHSTANDING the frequent occurrence of the remains of the gigantic elk in Ireland, it is remarkable that precise accounts should not have been kept of all the peculiar circumstances under which they occur entombed in its superficial strata. To obtain an opportunity of examining these relations had long been my desire; and as fortunately, during my avocations last autumn in the north of Ireland, a discovery came to my knowledge that seemed likely to throw light on the subject, I proceeded to its investigation, intending, should the results be found deserving of attention, to place them on record. These results have proved the more interesting, as they apparently lead to the conclusion, that this magnificent animal lived in the countries in which its remains are now found, at a period of time which, in the history of the earth, can be considered only as modern.

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* See Philosophical Magazine, vol. lxii. p. 81.

† From the Philosophical Transactions for 1825, Part II.

I had advanced thus far when I became apprised of an analogous discovery made last year in the west of Ireland, by the Rev. W. Wray Maunsell, archdeacon of Limerick; which is not only confirmative of my own experience, but has the additional value of embracing particulars not hitherto noticed by any other observer. Mr. Maunsell's researches, elucidated by the able assistance of Mr. John Hart, member of the Royal College of Surgeons, have been communicated from time to time to the Royal Dublin Society, in the form of letters, and have been entered upon their minutes; and it is to be hoped that a distinct publication on the subject may hereafter appear, illustrated by a description of the splendid specimen of the skeleton of the animal, now deposited by the liberality of the reverend archdeacon in the museum of that Society. In the mean time I propose, after giving a concise account of my own inquiries, to refer briefly to the more prominent points in Mr. Maunsell's discoveries, in as far as they bear immediately on the question of the ancient or modern origin of those remains.

The spot which I examined is situated in the county of Down, about a mile and a half to the west of the village of Dundrum. That part of the country consists of an alternating series of beds of clay-slate and fine-grained grauwacké, with occasional subordinate rocks, which it is needless at present to mention; the whole distinguished by numerous small contemporaneous veins of calcareous spar and quartz, and traversed in some places by true rake veins that are metalliferous. Hills of moderate elevation, from 150 to 300 feet high, are thus composed. In a concavity between two of these hills is placed the bog of Kilmegan, forming a narrow slip, which extends about one mile in a nearly N. and S. direction. The natural hollow which it occupies appears formerly to have been a lake, which in process of time became nearly filled by the continual growth and decay of marshy plants, and the consequent formation of peat. The latter, however, from the flooded state of its surface, afforded little advantage as fuel, until the present marquis of Downshire caused a level to be brought up from the eastward (part of it being a tunnel), and thus laid the bog dry. This measure was attended with a two-fold benefit to the tenantry,—the provision of a valuable combustible, and the discovery of an excellent manure in the form of white marl beneath the peat. The latter extends from a few feet to twenty feet in depth; and the subjacent marl from one to three, four, and five feet in thickness. The marl when fresh dug has partly a grayish tinge, but on losing its moisture it becomes white.

In cutting down the peat to the bed of marl, the remains of the gigantic elk have frequently been met with; and invariably, as I am assured by the concurrent testimony of the tenantry, placed between the peat and the marl; or merely impressed in the latter. It is stated that at least a dozen heads with the branches, accompanied by other remains, have thus been found from time to time: but being unfortunately deemed of no value by the country-people, they have for the most part been scattered and destroyed. It is to be hoped, however, that a sufficient inducement will lead them to bestow greater care on the preservation of whatever remains may be hereafter discovered.

The marl, upon examination, appears in a great measure composed of an earthy calcareous base, containing comminuted portions of shells: and that these are all derived from fresh-water species, is proved by the myriads of these shells that remain in the marl, still preserving their perfect forms. They are however bleached, very brittle, and retain little of their animal matter; but in all other respects they have the characters of recent shells. After examining several masses of the marl, I found the whole of the shells referable to three species,—two univalves, and one bivalve; namely,

1. The *Helix putris* of Linnæus. See Donovan's British Shells, pl. 168, fig. 1; and Lister, *Conch.* tab. 123, fig. 23.—N. B. Of the two, Lister's figure is the more exact representation of the shell.
2. The *Turbo fontinalis.* Donovan, pl. 102.
3. The *Tellina cornea.* Donovan, pl. 96.

Of these shells some prevail more in one spot than in another; but generally speaking, they appear distributed through the upper portion of the marl in nearly equal quantities; in the lower portion they are less frequent, if not altogether absent.

The circumstances which I have related seem to remove all idea of these remains of the Irish elk being of any other than comparatively recent origin. In seeking a cause for the nearly constant distribution of these remains in Ireland in swampy spots, may we not conjecture that this animal often sought the waters and the marshy land as a place of refuge from its enemies, and thus not unfrequently found a grave where it had looked for protection?

The foregoing conjecture appears supported by the following details of circumstances, observed by the Rev. Mr. Maunsell in the peat bog of Rathcannon, situated about four miles to the west of the town of Bruff, in the county of Limerick. This bog covers a space of about twenty plantation acres, occupying a small valley, surrounded on every side by a ridge of the carboniferous or mountain limestone, except on the S.W., where it opens into an extensive flat. The peat is from one to two feet thick; and beneath this is a bed of white shell-marl, varying from one foot and a half to two feet and a half in thickness, succeeded below by blueish clay marl, of an unascertained depth, but in one place it was found to exceed twelve feet. This blueish clay marl becomes white, and falls to powder on being dried. Coarse gravel is said to occur, partially at least, below the marl.

In this small valley portions of the skeletons of eight individuals were found, seven of adult, and one of a young animal, all belonging to the gigantic elk. With these also occurred the pelvis of an adult animal, probably referable to the red deer; and the skull of a dog, of the size of that of an ordinary water-spaniel.

The bones that were first discovered were found at the depth of two or three feet below the surface: and Mr. Maunsell had the advantage of seeing them before they were displaced. Most of the above-mentioned remains were lodged in the shell-marl; many of them, however, appeared to rest on the clay marl, and to be merely covered by the shell-marl. But part of some of the bones were immersed in the peat also: these were tinged of a blackish colour, and were so extremely soft, in consequence of the moisture they had imbibed, that it was with difficulty the horns found in this situation could be preserved entire; yet, when carefully handled and allowed to dry, they became as firm and hard as the rest.

Some of the bones of the elk showed marks of having been diseased; and one rib had evidently been broken, and afterwards reunited. Another rib exhibited a remarkable perforation of an oval form, about half an inch long and one-eighth of an inch broad, the longer axis being parallel to the side of the rib; the margin of this opening was depressed on the outer, and raised on the inner surface; while a bony point projected from the upper edge of the rib, which deviated from its natural line of direction to an extent equal to the length of the aperture. The only cause that could have produced this perforation is a wound by a sharp instrument, which did not penetrate deep enough to prove fatal, and between which event and the death of the animal a year at least must have elapsed, as the edges of the opening are quite smooth.

The bones are so well preserved, that in a cavity of one shank-bone which was broken, marrow was found, having all the appearance of fresh rendered suet, and which blazed on the application of a lighted taper. They appear to contain all the principles to be found in fresh bones, with perhaps the addition of some carbonate of lime, imbibed with the moisture of the soft marl in which they had lain.

The remains of the eight individuals were disposed in such a manner as to prevent the possibility of referring the component parts exactly to each skeleton; but all the heads with their branches were found; and one specimen is particularly fine, displaying the broad expanded palms, with almost every antler and projecting point in a perfect state. By joining this head to a selection from the other remains, a nearly perfect skeleton of the largest size has been formed by Mr. Hart; one rib, a few of the carpal and tarsal bones, and the bones of the tail being only wanting.

Of the shells found in the white marl many are preserved entire; but the greater part are broken into small fragments. They are all univalves, and belong to fresh-water species, which exist at the present day.

It is added, that so frequently have the remains of the fossil elk been discovered in the county of Limerick, that one gentleman enumerated thirty heads which had been dug up at different times within the space of the last twenty years.

From Professor Henslow's account of the curraghs, or peat bogs of the Isle of Man, it would appear that the remains of the gigantic elk are there also distributed in a manner analogous to that in which they are found in Ireland. That gentleman supposes a herd of elks to have perished there; and his description of the white, or grayish marl, in which their remains are found, answers in most respects to that of the white marl which so frequently forms the substratum of the peat bogs in Ireland.

Upon the whole, the preceding details appear to justify the conclusion that the extinction of the gigantic species of elk is attributable rather to the continued persecution it endured from its enemies, accelerated perhaps by incidental natural local causes, than to a general catastrophe which overwhelmed the surface of the globe. In a word, it may be inferred that these remains are not of diluvian, but of post diluvian origin.

Kenmare, April 12, 1825. T. WEAVER.

XXXI. *On the Ebullition of Water at Specific Temperatures, as the Measure of Altitude.* *By* JOHN MURRAY, *F.S.A. F.L.S. F.H.S. F.G.S. &c. &c.*[*]

IT is known that water boils in the attenuated atmosphere of the air-pump, at an inferior temperature, and that this point and period of ebullition has some ratio comparatively with the density of the incumbent air. Theodore de Saussure found that water boiled on the summit of Mont Blanc at 187° Fahr. It was Fahrenheit that first proposed this application of the thermometric expression of boiling water as a measure of altitude. In the Philosophical Transactions for 1817, the Rev. Francis Wollaston has described an instrument for this purpose, most ingeniously constructed, and no doubt accurate enough for minor elevations.

During last summer, on my excursion in Switzerland, Italy, &c., I made several experiments on the ebullition of water at different elevations. A few of these I beg leave to submit to you. The thermometer was graduated by a diamond on the stem; the bulb was small, and the divisions only indicated the entire degree of Fahrenheit's scale.

At the Hospice of the Great St. Bernard, on the 30th of July 1825, at eight o'clock P.M., the barometer indicated 21·08 inches. Thermometer without, 52° Fahr., and within the Hospice 59° Fahr. Water boiled at 186° Fahr.

At the village of Simplon on the Simplon, 13th of August, at ten o'clock P.M.; air 62°.

Exp. 1.—	Water boiled, ball touching the surface	197°	5′
	Ditto, entirely immersed	202	
	Ditto, bottom	203	5
Exp. 2.—	Ball touching the surface	197	5
	Ball immersed and at the centre . .	203	5
	Ditto at bottom . . .	205	

3d of August. At Brieg, in the Valais, at 4^{h} 30′ A.M.; air 56° Fahr. Water boiled at 204° 5′ Fahr.

15th of August. At Sion in the Valais, at ten o'clock P.M.; air 69° Fahr. Water boiled at 206° 5′ Fahr.

17th August. At Martigny in the Valais, six o'clock A.M.; air 57° Fahr. Water boiled at 110° Fahr.

1st of September. At the inn on the Mountain Righi, at 9^{h} 45′ A.M.; air 63° Fahr. Water boiled at 201° Fahr.

1st of September. At Lucerne, at 8^{h} 15′ P.M.; air 70° 5′ Fahr. Water boiled at 206° Fahr.

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[*] Communicated by the Author.

It will on proper calculation be seen that, though I pretend not to the niceties pointed out in Mr. Wollaston's ingenious paper, (the circumstances under which the experiments were made precluding such accuracy, and I had not indeed the entire provision of apparatus constructed by this philosopher,) ruder apparatus subserving my purpose,—that a distant approximation to the altitude, as indicated by the barometer at three elevations, is only insured.

In consequence of the capricious results indicated by my experiments on the ebullition of water at the village of Simpoln on the Simplon, I made a series of experiments with the thermometer on hot water contained in a tumbler. I subjoin the results of five of these experiments.

1.	Ball of thermometer touching the surface	131°	
	Ditto completely immersed	135	
	Ditto touching the bottom of tumbler	131	
2.	Ball in contact with the surface	131°	
	Ditto immersed	134	5′
	Ditto bottom	126	
3.	Ball on surface	124°	
	Ditto immersed	131	
	Ditto bottom	126	
4.	Ball on surface	116°	5′
	Ditto immersed	120	5
	Ditto bottom	116	5
5.	Ball on surface	109°	5′
	Ditto immersed	114	5
	Ditto bottom	109	5

In one experiment I found a difference of 1° 5′ Fahr. between the centre portion of the superior surface of the water and the sides. In another experiment, the difference amounted to 2° 5′ Fahr.

The following I presume to be the conditions that must interfere with anything like accuracy in thermo-barometrical indications of this kind in elevated regions.

1. The hygrometric state of the incumbent atmosphere at the time the observation is taken.

2. The attenuated pressure on the bulb of the thermometer, by which its form and dimensions must necessarily be altered.

3. The water used must be more expanded in volume at great altitudes, than on the level of the sea, its density being

therefore reduced in the ratio of the attenuated atmosphere. This density would be modified too by its saline contents.

4. The depth to which the ball of the thermometer is plunged in the water, and the place it occupies in the cylinder.

5. The evolving steam or heated vapour will also affect the stem of the instrument, and with it disturb the results.

6. The form, size, and material of the vessel will also contribute their share in modifying the indication.

7. The depth of the vessel containing the boiling water.

8. If made in the house, the difference between the internal and external temperature will be a modification of the phænomena.

9. A gust of wind, wafted from the glacier, avalanche, or other cooling surface, will disturb and change the density of the incumbent air; and therefore irregularities such as these must be provided for.

10. The greater or less rapid escape of the steam will necessarily render capricious the observed temperature.

11. The period of the day or night, strength of the sunbeams, &c.—all concur in varying the results.

It will, I apprehend, be very difficult to maintain a successful struggle against all these combining circumstances; and they thus render this instrument, certainly ingeniously applied by Mr. Wollaston, nearly useless for considerable elevations. Captain Hall's experiments corroborate the inference; and at those he made at the village of Simpoln on the Simplon, (with an instrument constructed under the immediate sanction of Mr. Wollaston,) in 1818, I had the satisfaction and pleasure of being present.

January 27, 1826. J. MURRAY.

XXXII. *Report made to the Academy of Sciences, 22d of August* 1825, *on the Voyage of Discovery, performed in the Years* 1822, 1823, 1824, *and* 1825, *under the command of M.* DUPERREY, *Lieutenant of the Navy**.

(*Commissioners: MM.* DE HUMBOLDT, CUVIER, DESFONTAINES, CORDIER, LATREILLE, DE ROSSEL; *and* ARAGO, *Reporter.*)

SINCE the return of peace, many voyages have been performed for the advancement of the sciences and of navigation. Captain Gauttier's maps of the Mediterranean and of the Black Sea; Captain Roussin's labours on the coasts of Africa and of Brazil; the expedition of Captain Freycinet; the hydrographic operations directed by our colleague Beautems-Beaupré, will be durable monuments of the enlightened protection which the minister of the marine affords to useful enterprises.

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* From the *Annales de Chimie*, tom. xxx. p. 337.

The plan of the new voyage, an account of which the Academy has charged us to give them, was presented to the marquis de Clermont-Tonnerre, then minister of the marine, by MM. Duperrey and Durville, towards the end of 1821. His Excellency approved of it, and placed the corvette Coquille at the disposal of these young officers. The zeal and skill of which they had given repeated proofs,—the one during the circumnavigation of the Uranie, the other as fellow-labourer of Captain Gauttier,—afforded every pledge that could be desired. The Academy will find, at least in our opinion, in the analysis which we have to lay before it of the numerous labours performed on board the Coquille, that the hopes of government and of men of science have been completely realized.

Itinerary.

The Coquille set sail from Toulon the 11th of August 1822. The 22d of the same month she anchored in the roads of St. Croix at Teneriffe, which she quitted the 1st of September, making for the coast of Brazil. In her passage, M. Duperrey observed, the 5th of October, the small isles of Martin-Vaz and of the Trinity; on the 16th, the Coquille was moored at the anchorage of the isle of Saint-Catherine: she staid there till the 30th. The 18th of November she reached Port Louis of the Malouines, situated at the bottom of the bay Française, from whence she sailed the 18th of December, to double Cape Horn: she then visited, on the western coast of America, the port Conception at Chili; that of Callao at Peru; and afterwards the port of Payta, situated between the magnetic equator and the terrestrial equator. The want of any diplomatic relation bebetween France and the republican governments of South America did not occasion any obstacle to the proceedings of M. Duperrey: on the coasts of Chili, as at Peru, the authorities eagerly complied with their slightest wishes.

The Coquille set sail from Payta the 22d of March 1823: in her course she coasted along the Dangerous archipelago, and first put in at Otaheite the 3d of May, and then at Borabora, which also makes part of the Society Isles. Quitting this last point, the expedition took a westerly course; observed, successively, the Salvage Isles, Eoa (in the group of the Friendly Islands), Santa-Cruz, Bougainville, Bouka, and reached New Ireland, where she anchored in the bay of Praslin the 11th of August.

After a stay of nine days, the expedition left the port of Praslin, to make for Waigiou. We shall presently speak of the observations which she made in her passage, and during her stay in the harbour of Offak, which she left on the 16th of September. On the 23d, M. Duperrey cast anchor at Cajeli, (Boron island); the 4th of October he landed at Amboina, where he received from M. Merkus, governor of the Moluccas, the most cordial reception, and all the assistance which he needed. On the 27th of October the Coquille again set sail, steering her course from north to south; she observed the isle of the Volcano; crossed the strait of Ombay; coasted the isles situated to the west of Timor; observed Savu, Benjoar, and finally left this latitude to make Port Jackson. Contrary winds did not allow M. Duperrey to range the western coast of New Holland, as he meant to have done: it was only on the 10th of January 1824 that he doubled the southern point of Van Diemen's land; the 17th, the corvette was moored in Sydney Cove. Sir T. Brisbane, governor of New Holland and corresponding member of the Academy, received our travellers with the most amiable eagerness, and put into their hands all that could contribute to the success of the operations with which they were entrusted.

In leaving Sydney the 20th of March 1824, after resting for two months, the expedition sailed for New Zealand, where it arrived the 3d of April, in the Bay of Isles. The works which were to be done there were terminated the 17th. During the first days of May, the Coquille had already surveyed in every direction the archipelago of the Carolines. The monsoon from the west obliged her to abandon these roads towards the end of June 1824; she then went to the northern extremity of New Guinea, ascertaining during the voyage the geography of a considerable number of islands little known or badly placed, and reached the haven of Dory the 26th of July; a fortnight afterwards she again sailed, to arrive, by crossing the Moluccas, at Java. She cast anchor in the port of Sourabaya the 29th of August; went from it the 11th of September; and arrived the following month at the Isle of France, where her operations detained her from the 31st of October to the 16th of November; she remained at Bourbon from the 17th to the 23d of the same month, and then made sail for Saint Helena. The stay of M. Duperrey in this island lasted a week. He went from it on the 11th of January 1825, cast anchor at Ascension the 18th, rapidly executed there the observations of the pendulum and of the magnetic phænomena, and finally quitted these English establishments on the 27th, after having received from the commanders and from the officers of the two garrisons every assistance that could be desired. At last, on the 24th of April, M. Duperrey entered the road of Marseilles.

During this voyage, of *thirty-one months and thirteen days*, the Coquille sailed 25,000 leagues. She came to the place of her departure without having lost one man, without illness, and without damage. M. Duperrey attributes for the most part the good health which his crew constantly enjoyed, to the excellent quality of the water preserved in the iron tanks, and also to the order which he had given that it should be used at pleasure. As to the good fortune which the Coquille had, to execute so long a voyage without damage either in its masts, its yards, or even in its sails, if it should be attributed to a concurrence of extraordinary circumstances which it would be imprudent always to expect, it should also be remarked that such chances only offer themselves to the best seamen. We may also add, that M. Duperrey and his fellow-labourers had had, in 1822, the advantage of finding at Toulon, M. Lefébure de Cerizy, an engineer of the greatest merit, who presided at the repair and outfitting of the corvette with all the solicitude of a true friend.

Maps and Plans taken during the Voyage of the Coquille.

The hydrographic works executed during the circumnavigation of the Coquille are already completely drawn, and only wait the hand of the engraver: they form 53 maps or plans, prepared in the best manner. We shall give in this place an enumeration, reciting the names of the officers to whom we are respectively indebted for them.

The plan of the islets of Martin Vaz and of the Trinity, on the coast of Brazil, has been executed with much care by M. Berard.

On the coast of Peru the same officer made a very detailed plan of the anchorage of Payta and a map of the adjacent coasts, from Colan, situated at a small distance from the mouth of the Rio de Chira, as far as the isle of Lobos.

The general map of the Dangerous archipelago has been executed by M. Duperrey himself; the particular map of the isle Clermont-Tonnerre belongs to M. Berard; the plans of the isles of Augier, Freycinet, and of Lostange have been made with much care by M. Lottin.

M. Duperrey has profited by his navigation among the Society Islands to rectify several serious errors which are remarked in all the maps of this archipelago.

M. Berard has taken, in the island of Otaheite, with his accustomed skill, the plan of the anchorage of Matavaï. The plan of the isles of Moutou-iti and Moupiti, and that of the anchorage of Papoa, are by M. Blosseville: they do equal honour to his zeal and his experience.

In New Ireland, Messrs. Berard, Lottin, and Blosseville have taken jointly and in the greatest detail the plan of Port Praslin and of the creek belonging to the English, the plan of Cape Saint George, and the chart of the Strait of the same name which separates New Ireland from New Britain.

In quitting New Ireland, the Coquille made a detailed survey of the isles of Schouten, respecting which we had hitherto only rather confused notions. M. Duperrey made the chart of it. The harbour of Offak, in the isle Waigiou, of which the interior was little known, has been the object of peculiar labour, in which all the officers took part. M. Berard made the chart of that portion of the coast of New Guinea lying between Dory and Auranswary; the plan of the harbour of Dory is founded on the united observations of Messrs. Berard, Lottin, and De Blois. The chart of the coast between Dory and the Cape of Good Hope in New Guinea, is by M. Lottin. It is also to this officer we owe the map of the isles of Yang, situated to the north of Rouib.

Cruisings performed in very various directions in the Moluccas have furnished M. Duperrey with the elements of a new chart of this archipelago, and of that of the strait of Wangi-Wangi, to the east of the isle of Boutoun. Admiral D'Entrecasteaux saw only the northern coasts of the islands Savu and Benjoar, situated to the south-west of Timor; M. Berard has traced a great part of the southern coasts. The chart of the strait of Ombay and of the island of the Volcano is also formed upon the observations of the same officer. That of the island of Guébé is due to M. de Blois.

In New Zealand, the labours of the Coquille had for their object the northern extremity only of the island Eaheinomauve; they occupy four plates. The first shows the configuration of all the N.E. coast: it is by M. de Blois. The second represents the Bay of the Isles, from the united labours of all the officers. The third gives the plan of the Bay of Manawa, by M. Berard. And the fourth, is the detailed plan of the river Kédékédé, laid down after the observations of M. de Blosseville.

The isolated islands of Rotumah, Cocal, and Saint-Augustin were taken by Messrs. Berard and Lottin.

In the archipelago of the Mulgrave Islands, the general chart of which M. Duperrey has drawn, M. de Blosseville has completed a survey of King's Mill, Hopper, Wood and Henderville islands; and M. de Blois that of Hall's Island; of an archipelago of five islands; and lastly, of the Mulgrave Islands, properly called Marshall's Islands.

The vast archipelago of the Carolines, hitherto so imperfectly known, has been the principal theatre of the geographic operations of the Coquille. The general chart of it which M. Duperrey has made will rectify many errors. Benham Island is there represented according to the observations which M. de Blosseville made. Ualan Island, which the American Captain Crozier named Strong, and to which M. Duperrey has restored the name which the inhabitants give it, merits particular interest. During a stay of fifteen days, the officers of the corvette went over it in every direction; they found there some tolerably large ports: one, which the inhabitants call *Lélé*, and another which has received the name of the *Coquille*, are laid down in the atlas, after the very detailed operations of Messrs. Berard, Lottin, and de Blois.

M. de Blois has besides made a complete survey of the islands Tougoulon and Pelepap, which are probably the MacAskill of certain maps; and also of the islands Mougoul, Ougai, and Aoura, which were discovered on the 18th of June. It is also to this officer we owe the detailed plan of the rather extended group of Hogoleu, of which father Cantova had already formerly spoken; and in the midst of which the Coquille navigated, the 24th of June 1824. The survey made by M. Lottin of the islands Tametain, Fanadik, and Holap, unites in these latitudes the operations of the Coquille to those of the Uranie.

The three last sheets of this rich atlas, an analysis of which we have just given, represent the anchorages of Saint-Helena and of Sandy Bay, and the island of Ascension, from the observations of all the officers.

Charts are not the less improved, when freed from islands, rocks and sand-banks which do not exist, than when newly discovered lands are inserted in them. The expedition of the Coquille will have rendered more than one service in this respect.

According to most geographers, there is, not far from the eastern coasts of Peru, a rock named the Trepied. M. Duperrey has sought for it in vain: the Coquille went full sail over the very places where the Trepied is generally laid down.

Whilst standing along the coasts of New Guinea, M. Duperrey sought with great care, but without success, for the isles which Carteret had named Stephens' Islands. According to him, these islands, still represented in our maps, would be the Providence Islands of Dampier, situated at the opening of Geelving Bay: this is also the opinion of Captain Krusenstern, and it cannot be denied that it is now a very probable one. It will nevertheless seem very strange that Carteret should have been deceived by nearly three degrees in his reckoning.

Our most modern maps place a group of isles called the

Trials, opposite De Witt's land, by 20° of south latitude and 100° west longitude; M. Duperrey, who would have attached a great value to the determination of their position, was not able to find them.

The archipelago of the Carolines was repeatedly sailed through and minutely examined. M. Duperrey shows satisfactorily that Hope island, Teyoa island, the groups of Satahual and Lamurek, do not exist in the positions which are assigned to them. Perhaps it may be sometimes difficult for him exactly to apply these old names to the islands whose place he has fixed. Moreover, the inconvenience is not serious; all was so inexact in the charts of this archipelago, that the labours of the Coquille are equivalent to a first discovery.

Astronomical Observations.

In a voyage like that of the Coquille, in which the periods of lying-to were always necessarily very short, the astronomical observations could only have for their object the improvement of geography. These observations, in each port, consist of elevations of the sun and stars fit for verifying the rate of chronometers; of numerous series of circummeridian heights taken with the astronomical repeating circle, and designed for giving the latitudes. Lastly, of a multitude of distances from the moon to the sun, to the stars and to the planets, taken with the reflecting repeating circle.

The examination which we have made of the part of this labour already completely reduced, has given us a most favourable opinion of it. All the officers of the Coquille have equally assisted in it. We must here, however, make particular mention of M. Jacquinot, who, intrusted by the commander with the care of the chronometers during the whole voyage, fulfilled this critical task with a zeal and exactitude worthy of the praises of the Academy.

Observations relative to the Determination of the Figure of the Earth.

M. Duperrey was furnished with two invariable pendulums of copper, which had before served in the voyage of the Uranie. They had been observed at Paris before the departure, and after the return of the expedition; at Toulon, whilst the vessel was fitting out; at the Malouines, 51° 31′ 43″ south latitude; at Port Jackson, on the eastern coast of New Holland; at the Isles of France and Ascension, between the tropics. Our colleague, M. Mathieu, has already calculated the observations for the Malouines and those of Paris. He has deduced from them this important consequence, at variance with an opinion long accredited, that the two terrestrial hemispheres north and south have very nearly the same form. Those of the observations which there has not yet been time to discuss, belong to questions not less curious. It results, for example, from the operations of M. Freycinet, that there exists at the Isle of France a cause of local attraction so intense as to alter the rate of a clock there 13 or 14 seconds a day. It may be conceived how interesting it becomes to investigate, in the observations of M. Duperrey, if the accidental influence was also manifest.—In a few days the results of this inquiry will be presented to the Academy.

[To be continued.]

XXXIII. *List of Errata in the Mathematical Tables of Dr.* HUTTON *and Dr.* GREGORY. *By Mr.* J. UTTING.

To the Editor of the Philosophical Magazine and Journal.

Sir,

AS it is very desirable to obtain the greatest accuracy in mathematical tables, the following list of *errata*, which I have discovered in Dr. Hutton's and Dr. Gregory's tables, will I trust be acceptable to such gentlemen as use the tables in which the following list of errors are pointed out.

In Dr. Hutton's Mathematical, &c. Dictionary, first edition:

Square roots of numbers to ten places of decimals.

Nos. 138	*for*	11.	43808	*read*	11.	01245
149	—	12.	3	—	12.	7
197	—	14.	41	—	14.	76
374	—	19.	537514	—	19.	796058
462	—	21.	579	—	21.	602
482	—	21.	24	—	21.	01
499	—	22.	9	—	22.	7
504	—	22.	1206	—	22.	3206
580	—	24.	683902	—	24.	891576
586	—	24.	6	—	24.	8
634	—	25,	01	—	25.	40
706	—	26.4		—	26.5	
712	—	26.	3	—	26	2
788	—	28.	881	—	28.	952
879	—	29.	24743	—	29.	41607
952	—	30.	7	—	30.	1

For Dr. Hutton's tables of the product and powers of numbers:

Table of products.

No. 15 by 277, *for* 5155 *read* 4155.

In the table of cubes.

Nos. 11	*for*	1338	*read*	1331
408	—	67911312	—	67917312
702	—	345948008	—	345948408
813	—	537366797	—	537367797

The last 3 errors apply also to Dr. Hutton's *Course* and *Tracts.*

In Dr. Gregory's Mathematics for Practical Men:

Table II. of Supplementary Tables.

In the column of Areas.

Nos. 7	*for*	38. 6000	*read*	38. 1001
18	—	264. 46900493	—	254. 46900494
19	—	6	—	9
24	—	07	—	12
28	—	7	—	0
33	—	89	—	94
40	—	4143	—	6144
56	—	68	—	41
61	—	92	—	00
64	—	0	—	8
65	—	0	—	5
96	—	0	—	7

In the areas for Nos. 22, 27, 30, 32, 39, 45, 48, 51, 54, 57, 60, 62, 66, 69, 72, 75, 87, 90, 99, increase the last figure by 2.

In Nos. 1, 8, 9, 10, 14, 16, 20, 21, 23, 26, 29, 34, 36, 38, 41, 42, 44, 46, 47, 49, 50, 52, 55, 58, 59, 63, 68, 70, 71, 74, 78, 81, 84, 85, 86, 88, 89, 92, 93, 94, 95, 97, and 98, increase the last figure by unity.

N. B. The areas for each integer, from 1 to 100, or one-twelfth part of this table only, has been examined.

I have recomputed the Tables of Dr. Hutton, for all Nos. from 1 to 1000; and if the above corrections are made, the tables to which they apply will stand correct.

March 1826. J. Utting.

XXXIV. *On the Phænomena connected with some Trap Dykes in Yorkshire and Durham. By the Rev.* Adam Sedgwick, *M.A. F.R.S. M.G.S. Fellow of Trinity College, and Woodwardian Professor in the University of Cambridge**.

Introduction.

THE various phænomena presented by trap rocks have long engaged the attention of geologists. Different ages have been assigned to them, founded on their union with older or newer strata, and distinctive characters have been pointed out by which it has been attempted to separate the several formations from each other. As observations have become more widely extended, many of the conclusions founded on such characters have proved to be fallacious; and it is now generally admitted, that the mineralogical composition of any system of trap rocks gives us little information respecting its antiquity or probable associations. When strata rest conformably upon each other, in such a way as to indicate a continued succession of depositions, we can immediately determine, at least, their *relative* antiquity, and may often adopt some natural or artificial arrangement which will greatly facilitate their description. But formations, which appear as dykes and overlying masses, afford no such facilities for correct classification; and the only general conclusion which we can arrive at respecting them is, that they are newer than the beds into which they have intruded. It is on this account that different observers have formed completely different views respecting the classification of certain formations of trap; each, in ambiguous cases, having adopted that opinion which happened to fall in with his favourite theory.—In determining the origin of any one of these formations, it seems essential to inquire, (1) In what manner it is associated with other rocks. (2) What minerals enter into its composition. (3) What effects are produced by its presence. Satisfactory answers to these questions have been obtained from so many quarters, that the discussions in which they have originated will perhaps soon terminate. It is my intention in this communication to bring together some facts, connected with the subject, which fell under my observation during last summer.

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* From the Cambridge Philosophical Transactions, vol. ii. Part I.

Trap Dykes in the Coal-fields.

Dykes and overlying masses of trap are of such ordinary occurrence in many of our coal-fields, that they have sometimes been regarded as true members of the great coal formation. Should it, however, appear, that they have not originated in the same causes which formed those innumerable layers of sandstone, shale, ironstone, &c. which enter into the composition of the coal strata; but that they have been subsequently driven in among these beds by the irregular action of powerful disturbing forces; we shall then be compelled to regard them, not as the subordinate members, but as the intrusive associates of the great coal formation. In confirmation of this opinion it may be stated; (1) That in many extensive coal-fields there are no traces of any beds or dykes of trap. (2) That in other places, such beds or dykes pass beyond the bounds of the coal-fields, and traverse indifferently all the newer strata which cross their line of direction. The facts presented by the north coast of Ireland afford several illustrations of the truth of this assertion.

Mr. Winch, in the fourth volume of the Geological Transactions, has given many interesting details respecting the dykes* which intersect the great coal basin of Northumberland and Durham. They are in some instances filled with clay and rounded pebbles or shattered fragments of sandstone, mixed with other materials derived from the neighbouring rocks, and their whole appearance plainly indicates the violent nature of the forces by which the solid strata have been cleft asunder. In other instances, the fissures are filled with a variety of basalt, which rises like a great partition wall through all the beds of the formation. (Geol. Trans. vol. iv. p. 21—30.) It is the opinion of Mr. Winch that these basaltic dykes never pass up into the magnesian limestone which reposes immediately on the coal strata. Thus, for example, the cliff of Tynemouth castle is intersected by a basaltic dyke which does not penetrate the capping of magnesian limestone.

Every one who is acquainted with the details of English geology must have remarked, that our newer strata, down to the magnesian limestone inclusive, are generally unconformable to all the older rocks. Thus in numberless instances, more especially in the West of England, we find some of the newer strata filling up the inequalities, or resting on the inclined edges, of the coal measures. In all such cases, the fractures and contortions of the lower formation must have taken place prior to the deposition of the superincumbent horizontal beds. Now if it appear, that masses of trap are not only the common associates of such fractures and dislocations, but sometimes the very instruments by which they have been produced; it follows, almost of necessity, that the dykes we have been describing will not generally be found among the horizontal beds which repose upon the disturbed strata. Such a rule as this may, however, admit of many exceptions. For no reason can be given *à priori*, why the same forces, which produced the great fissures in our coal formations, should not again come into action in successive epochs in the natural history of the earth. Accordingly, it is found that basaltic dykes are not confined to any particular set of strata, but may occasionally appear among the newest secondary rocks. The facts exhibited by the north coast of Ireland have been already alluded to. The great dyke which starting from Cockfield Fell, in the county of Durham, crosses the plain of Cleveland, and terminates in the eastern moors of Yorkshire, leads us to a similar conclusion.

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* In the North of England the term *dyke* is not confined to the description of those fissures which have been filled with trap, but is extended to all the great faults and dislocations which intersect the strata in a nearly vertical direction. A want of attention to this extended use of the word has given rise to occasional mis-statements and false inferences.

Cockfield Fell and Cleveland Dykes.

This dyke, which preserves such an extraordinary continuity, forms a striking feature in all the geological maps of the district. Some good general descriptions have already been given of it *. My principal object in this paper will be, to place before the Society, in a connected point of view, those facts which appear to bear on the question of its origin. I shall afterwards notice some phænomena which are exhibited in High Teesdale, and seem to throw light on the same question.

Dykes near Egglestone in Upper Teesdale.

A mass of trap occupies the lower part of the left bank of the river Tees exactly opposite to the entrance of the Lune. It may be traced without difficulty for three or four hundred feet, close to the edge of the water; and it at length disappears under Egglestone bank; where it rests upon, or abuts against a bed of slate clay. The prolongation of the trap to the other side of the Tees is rendered highly probable by the appearance of a bed of similar character in the left bank of the Lune immediately under Lonton Chapel. But the accumulation of diluvium prevents this connexion from being established by direct evidence. The imperfect denudation on the left bank of the Tees did not allow me to ascertain the exact relation which the trap on that side of the water has to the contiguous strata. Above Egglestone bank another mass of trap, to all appearance immediately connected with that which has been described, crosses the road about a mile to the northwest of the village. It there assumes the unequivocal characters of a dyke, ranges (as nearly as I could discover from very imperfect data) E. by N. and a few hundred yards above the road crosses the western branch of the rivulet which runs past Egglestone. A quarter of a mile further up the same branch of the rivulet, a second dyke crosses its bed, and seems to range about S.E. by S. From what has been stated it appears probable that these two dykes unite, or intersect each other. Their concourse will probably be found on the moor above the new smelting-house. The former, where it is seen above

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* Geological Survey of the Yorkshire Coast, by Young and Bird. p. 171.

Egglestone, is about forty feet wide, and cuts through a bed of coarse grit, provincially called *firestone*. The latter is about sixty feet wide, and is associated with gritstone and a band of indurated shale which has been much quarried for whetstones.

It would certainly be very interesting to trace these dykes as far as possible through the eastern moors, as there can be little doubt of their connexion with some of those masses of trap which traverse the great coal-field. My own observations were much too limited to complete this task. I however found on Woolly Hills, in the Woodland Fells, several quarries opened in a dyke which, from its position as well as in its structure, seemed to form a connecting link between the trap of High Teesdale and some of the dykes which traverse the country near Cockfield Fell *.

Cockfield Fell Dyke.

Proceeding some miles further to the S.E. we come to the north-western termination of Cockfield Fell dyke, which is seen in a quarry by the side of the brook which runs past Gaundlass Mill. In that single locality it assumes a compound form, being made up of three distinct and nearly vertical masses of trap alternating with a variety of indurated slate-clay. The following is a transverse horizontal section of the whole dyke. (1) On the south-west side, common coal shale, which, as it approaches the dyke, becomes much indurated and has a vertical cleavage. In this state it is provincially termed *pencil*. (2) Trap one yard. (3) *Pencil* about four or five yards, but of variable thickness and much shattered. (4) Trap two yards. (5) *Pencil* half a yard. (6) Trap about seven yards. (7) Coal shale resembling No. (1). These entangled masses of coal shale are probably not prolonged far beyond the quarry, as they are seen in no other section.

The dyke afterwards ranges through the coal works which are opened in Cockfield Fell about half a mile to the north of the village; and its further progress in a direction about E.S.E. is marked in Blackburn quarry and Crag-wood. Near the former place it is intersected by a cross course, and heaved several yards out of the line of its direction. To the S.E. of Crag-wood, it would perhaps be impossible to trace it at the surface; but the vein of trap which runs along the high ridge of coal strata between Bolam and Houghton-le-side, agrees so well in character and direction with the masses above mentioned, that it has generally been assumed as the prolongation of them.

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* It is stated by Mr. Winch (Geological Transactions, vol. iv. p. 76.), that "at Egglestone, three miles below Middleton, a very strong vein of basalt may be seen crossing the Tees in a diagonal direction." I suspect that he here alludes to the mass of basalt abovementioned, which appears on the left bank of the Tees opposite to the entrance of the Lune, as I in vain endeavoured to discover the traces of a dyke further down the river. If this conjecture be right, it will be necessary to remove the dyke (which in the map accompanying Mr. Winch's memoir is made to cross the Tees below Egglestone) to a place considerably to the N.W. of its present position. When so represented, it will be seen, by an inspection of the map, that the basalt in Teesdale and the neighbourhood of Cockfield Fell are much more nearly in a straight line than they have been represented.

Bolam Quarry.

In the quarries which they are now excavating near Bolam, the vertical dyke is unusually contracted in its dimensions; but on reaching the surface, it undergoes a great lateral extension, especially on the south-west side, so that the works are conducted in a perpendicular face of columnar trap more than two hundred feet wide. The changes produced by this overlying columnar mass are highly instructive, and will be described in their proper place. The old excavations, in the direction of Houghton-le-side, show that the trap is there confined to a fissure nearly forty feet wide, which, with a slight undulation in its direction, bears to a point about S.E. by E.

Sandstone on the Trap.

There is another locality, the mention of which must not be omitted, though I think it probable that it is not in the line of the great dyke. In this opinion I may, however, have been misled by the maps of the district, in which many of the places are laid down entirely out of their true bearings. At Wackerfield-lane-end, half a mile W.N.W. of Hilton, a mass of trap appears to range east and west, and may therefore join the leading dyke which intersects the country still further to the east. The excavations in that place would not deserve any particular attention, were it not for the important fact, that at their western termination horizontal beds of sandstone are seen to rest immediately upon the upper surface of the dyke. I have been informed that masses of trap occur on the north-east side of the quarries of Bolam; but I had no opportunity of examining them with a view of ascertaining their probable connexion with the principal dyke.

From all these facts we may infer—(1) That from Gaundlass Mill to Houghton-le-side, a distance of about ten miles, the dyke of trap is uninterrupted—(2) That it may be connected with other dykes, which appear still further to the north-west nearly in the same line of direction, and through them with the dykes in Upper Teesdale—(3) That it probably gives out some lateral branches connecting it with other masses of trap in the same district. It may further be observed, that all this portion of the dyke, however modified by local circumstances, dips towards a point on the north-eastern side of its general line of direction, so as to make with the horizon an angle perhaps in no instance less than eighty degrees.

The high ridge of coal strata, extending from Bolam to Houghton-le-side, forms a kind of abutment which encroaches considerably on the line of the magnesian limestone. The present collocation of the two formations might lead to a conjecture that a great fault, ranging along the line of demarcation, had thrown the magnesian limestone down below its natural level. But the supposition is not necessary; for the appearance of the limestone below the level of the ridge may be only an indication of its unconformable position.

Dyke in Lower Teesdale.

In the low region of the magnesian limestone we lose all traces of the basalt from Houghton-le-side to Coatham Stob. From the last-mentioned place it may be traced through the quarries of Preston across the Tees; and very large excavations have been made in a corresponding quarry at Barwick on the right bank of the river. The mineralogical character of this dyke, its direction, and its dip, agree so well with the one which ranges through Cockfield Fell; that no one has, I believe, denied the probability of their being continuous *. The great distance between Houghton-le-side and Coatham Stob in which no trap has been discovered; and still more the fact, that the basaltic veins in the great coal-field do not generally pass up into the magnesian limestone; have led some to imagine, that the prolongation of the dyke of Cockfield Fell is for several miles concealed beneath the beds of that formation. These basaltic veins, which do not penetrate the magnesian limestone, prove one of two things. Either that they took their present form before the deposition of the limestone; or that they were injected from below, but not with sufficient energy to break through the superincumbent limestone.—Neither of these suppositions can apply to a great dyke intersecting an enormous mass of secondary strata which are newer than the magnesian limestone, and probably rest upon it. If there-

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* Should any one maintain that the dykes of Cockfield Fell and the plain of Cleveland have a distinct origin; he may, perhaps, draw an argument in favour of his own opinion, from the great thickness of the vein of trap in the quarries of Preston, Barwick, Langbargh, &c. In this one respect there is undoubtedly a considerable difference between them.

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fore we admit the identity of the Cockfield Fell and Cleveland dykes; we must suppose that in the whole interval, between Houghton-le-side and Coatham Stob, it is concealed by a thick covering of diluvium: an opinion which no one will have much difficulty in admitting who has observed the enormous accumulation of transported materials in all the neighbouring district.

Range of the Dyke through the Eastern Moors.

At Preston the trap emerges from beneath nearly fifty feet of diluvian brick earth; and would probably have remained concealed, had it not been laid bare in the bank of the river. On both sides of the Tees it is more than seventy feet wide, and ranges through horizontal strata of sandstone in a direction about S.E. by E. These horizontal strata must be referred to the new red sandstone formation, though they exhibit but faint traces of the usual ferruginous tinge. From Barwick, the dyke passes through the quarries of Stainton, Nunthorp, and Langbargh, to the foot of the Cleveland hills; making in its progress a considerable flexure to the north. At Stainton, the north face of the dyke is interrupted by a fissure about five feet wide, which is filled with light coloured argillaceous materials, with a transverse slaty texture. These substances bear no resemblance either to the sound or decomposing specimens of the dyke itself.

On the east side of Nunthorp it gradually rises above the level of the neighbouring country, and might be mistaken for a gigantic artificial mound, had not the quarries exposed its interior structure. A well defined ridge, about four hundred feet above the level of the neighbouring plains, marks its passage over the south flank of Rosebury Fopping. Still further to the east it is traced by a gap in the outline of the moors: for the upper beds of sandstone appear to have been shattered and carried off, and the dyke only rises to the highest level of the great bed of alum-shale. After passing through this gap and descending into Lownsdale, we found the trap forming a mass of bare rock which rose twenty or thirty feet above the vegetable soil. From thence it may be followed without difficulty many miles down the valley of the Esk, in a line bearing about E.S.E. Afterwards, by the turn of the valley at Egton Bridge, it is once more brought through the high moorlands; and its course is marked in that desolate region by a low ridge resembling an ancient Roman road. A quarry which is opened at Silhoue, near the seventh milestone on the road from Whitby to Pickering, proves the whole thickness of the dyke to be about forty feet, and its inclination and direction nearly the same as in the other localities. Beyond this place it continues to thin off; but it may be traced, though not without some difficulty, as far as a small rivulet about two miles to the east of the road. The exact point of its termination has perhaps not been ascertained; but there does not seem to be any good reason for supposing that it is continued to the German Ocean, as no vestige of it has been seen in any part of the cliff where it might be expected to appear.

[To be continued.]

XXXV. *Notices respecting New Books.*

THE First Part of the Second Volume of the Memoirs of the Astronomical Society has just been published, and the following are its contents:

On the method of determining the difference of meridians, by the culmination of the moon. By Francis Baily, Esq.—On the utility and probable accuracy of the method of determining the sun's parallax by observations on the planet Mars near his opposition. By Henry Atkinson, Esq.—On the corrections requisite for the triangles which occur in geodesic operations. By Captain George Everest.—On the rectification of the equatorial instrument. By J. F. Littrow.—On the variation in the mean motion of the comet of Encke, produced by the resistance of an ether. By M. Ottaviano Fabrizio Mossotti.—Observations of the solstice in June 1823, made at Paramatta, New South Wales. By Sir Thomas Brisbane.—Observations made in the years 1823-4 at Paramatta, New South Wales. Transmitted by Major-General Sir Thomas Brisbane.—On a new instrument, called the Differential Sextant, for measuring small differences of angular distances. By Benjamin Gompertz, Esq.—Observations on some singular appearances attending the occultation of Jupiter and his satellites on April 5, 1824. By Mr. Ramage, Captain Ross of the Royal Navy, and Mr. Comfield.—Observations on the occultation of the Herschel planet on August 6, 1824. By Capt. John Ross.—An account of the arrival and erection of Fraunhofer's large refracting telescope at the observatory of the Imperial University at Dorpat. By Prof. Struve.—On a new zenith micrometer. By Charles Babbage, Esq.—The results of computations on astronomical observations made at Paramatta, in New South Wales, under the direction of Sir Thomas Brisbane, and the application thereof to investigate the exactness of observations made in the northern hemisphere. By the Rev. John Brinkley, D.D.—A short account of a new instrument for measuring vertical and horizontal angles. By George Dollond.

Dollond, Esq.—Observations made at Bushey Heath (north latitude $51^\circ\ 37'\ 44''\cdot3$; west longitude, in time, from Greenwich, $0^{h}\ 1^{m}\ 20^{s}\cdot93$), from May 17, 1816, to December 7, 1824. By Colonel Beaufoy.—On astronomical and other refractions; with a connected inquiry into the law of temperature in different latitudes and at different altitudes. By Henry Atkinson, Esq.—A report on the properties and powers of a new 3-feet altitude and azimuth circle, lately fixed at the Rectory-house of South Kilworth in the county of Leicester: constructed by Edward Troughton, and divided by T. Jones. Drawn up by the Rev. William Pearson, LL.D.—Observations made at Paramatta, in New South Wales, by Major-general Sir Thomas Brisbane. To which are annexed, Observations made by Mr. C. Rumker, at Stargard, New South Wales, on the comet which appeared in July 1824.—Astronomical observations: 1. Observation of an eclipse of the moon, taken at Chouringhy, near Calcutta, in the year 1798; and, 2. Observations of the eclipses of Jupiter's satellites, taken at Chouringhy, in the years 1797, 1798, 1799, 1800, 1801, and 1803. By the late Colonel R. H. Colebrooke; 3. Observations of the eclipses of Jupiter's satellites, taken at Chouringhy, in the years 1821, 1822, and 1823. By Captains Hodgson and Herbert; 4. Observations of the occultations of the Pleiades by the moon, in July and October 1821. By the Rev. W. Pearson, LL.D.—Report, lists of presents, members, associates, and officers: Appendix, containing a part of the tables (mentioned in our last Number, p. 138) for determining the apparent places of nearly 3000 principal fixed stars: with a treatise on their construction and use, drawn up at the request of the council, by the president, F. Baily, Esq.

Just published.

The Narrative of a Tour through Hawaii or Owhyhee; with an account of the geology, natural productions, volcanos, &c. history, superstitions, traditions, manners and customs of the inhabitants of the Sandwich Islands; a grammatical view of their language, with specimens. The account given of the death of Captain Cook by the natives, and biographical notices of the late king and queen who died in London. By W. Ellis, missionary from the Society and Sandwich Islands.

The Tanner's Key to a New System of Tanning Leather quicker and cheaper than usual.—Price 5*s.*

XXXVI. *Pro-*

XXXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 23.—A Paper was read, entitled, "An Account of a new reflecting curve; with its application to the construction of a telescope having only one reflector;" by Abram Robertson, D.D. F.R.S. Savilian Professor of Astronomy, Oxford.

Also a paper, on the constitution of the atmosphere; by John Dalton, Esq., F.R.S.

Mar. 2.—Two papers by Sir E. Home, Bart. V.P.R.S., were read, on the coagulation of blood by heated iron.

Mar. 9.—A paper was read, on oil of wine; by Mr. H. Hennell: communicated by W. T. Brande, Esq. Sec. R.S.

A paper was also read, on the mathematical principles of suspension bridges; by Davies Gilbert, Esq. M.P. V.P.R.S.

The reading was commenced of a paper on a new method of determining the parallax of the fixed stars; by J. F. W. Herschel, Esq. Sec. R.S.

Mar. 16.—The reading of Mr. Herschel's paper was concluded; and a paper was read, on the expression of the parts of machinery by signs; by C. Babbage, Esq. F.R.S.

The Society then adjourned till April 6th.

LINNÆAN SOCIETY.

Mar. 7.—A further portion of Dr. Hamilton's Commentary on the *Hortus Malabaricus* was read.

Mar. 21.—The following communications were read:—

Descriptions of two new birds belonging to the family *Phasianidæ*, by Major-gen. Hardwicke, F.L.S.

The first of these birds is a species of the genus *Lophophorus* of M. Temminck, which General Hardwicke proposes to call *L. Wallichi*, after Dr. Wallich, the distinguished curator of the Company's botanic garden at Calcutta, through whose exertions, aided by the influence of the Hon. Edward Gardner, the English resident at the court of Katmandu, many interesting subjects in ornithology were procured. It is about the size of the Impeyan Pheasant, another species of *Lophophorus*, to which it does not yield in beauty. It is a native of the Almorah Hills on the north-eastern boundary of Bengal. The local name of this bird is Cheer.

The second species belongs to *Phasianus*, and will together with *P. cruentus* constitute a small but well marked group of that interesting genus. General Hardwicke has called this species *P. Gardneri*. It is a native of the Snowy Mountains north of the valley of Nepal.

Description of a new genus belonging to the natural family of plants called *Scrophularinæ*, by Mr. David Don, Libr. L.S.

Mr. Don proposes to name this genus *Lophospermum*, and in this paper points out its affinity to *Antirrhinum* and *Maurandia*, from both which, however, it is abundantly characterized by its flat winged seeds and campanulate corolla. The essential characters of the genus are as follows:—*Calyx* 5-partitus. *Corolla* campanulata: *limbo* 5-lobo, subæquali. *Capsula* bilocularis, irregulariter dehiscens. *Semina* imbricata, membranaceo-alata.

The genus consists of two species, both of them natives of Mexico, where they were discovered by the Spanish botanists Sessé and Mocinno, and which Mr. Don has named *Lophospermum scandens* and *physalodes*.

A review of the genus *Combretum*, by Mr. George Don, A.L.S.

The author here describes thirty-eight species of this interesting and beautiful genus, exclusive of six doubtful species enumerated by Dr. Roxburgh in the *Hortus Bengalensis*. In the *Systema Vegetabilium* of Professor Sprengel, which is the latest general work, only six species are enumerated.

GEOLOGICAL SOCIETY.

March 3.—The reading of Sir A. Crichton's paper On the Tanuus Mountains in Nassau was concluded.

The great mountain groups forming the Tanuus, are portions of that vast chain which crosses the Rhine to Valenciennes; and in the duchy of Nassau they are composed of transition and trap rocks: they here separate into two ranges, nearly at right angles to each other. The southern chain lies on the north of Mayence and Frankfort, and its highest point is the Feldberg, 2600 feet above the level of the Mayne. The northern chain includes the Westervald, celebrated for its brown coal. The strata of the southern face of the former chain, consist of talc and quartz-slate dipping north-west; whilst those of the northern face are of grauwacké and clay slate, inclining upwards south-east. The summit is a decomposing quartz rock, containing talc and iron, the sides and base of the mountain being formed of talc and slate. The baths of Schlangenbad are surrounded by slaty quartz: quartz conglomerates occur near the foot of the southern chain; where also a thick bed of sandstone, resembling our new-red-sandstone, rests upon the calcareous deposits of the valley of the Mayne, quarries of which are seen at Wisbaden.

The valley of the Mayne, which is interposed between the northern and southern chains, is chiefly occupied by low hills of coarse shelly limestone, analogous to the upper fresh-water formation of Paris, and quarries of it occur near Wisbaden and Hockheim: *Paludinæ* and *Modioli* abound in it. At Hockheim the beds are much dislocated; and at Wisbaden fossil bones are found, the teeth accompanying which refer them to animals allied to the *Lophiodon tapiroides*, and to the Sumatran Tapir. These calcareous deposits are only two hundred feet above the level of the Mayne, and they are perforated in many places by basalt, upon which they rest. The basalt finally disappears south-east of Darmstadt, and is succeeded by primitive rocks. There are strong salt-springs at Soden, and various mineral waters near Frankfort and Hadnigstein.

The Falkenstein mountain, though composed of talc-slate, protrudes through the high table land in the form of basalt. To the north of this the older rocks disappear, and the district is occupied by grauwacké. The grauwacké is divided into quartz grauwacké and grauwacké slate; the latter is very distinct from micaceous slate, and contains casts of *Spiriferi*, of the *Pleurobranchi* of Cuvier, &c.; the former offers encrinites, and unknown coralloids. The valley of the Lahn, between Coblentz and Diety, affords the best sections of grauwacké, and higher up that river the transition limestone appears at Baldowinstein. The schalstein (or problematic stone of Von Buch), is seen in all its varieties in the valley of the Aar, and with it are associated, porphyry, carbonate of lime in veins, iron, and copper. At Diety and Baldowinstein, porphyry seems to rise through the limestone. Crystalline dolomite, resting upon transition limestone, is the most recent formation observable in the mountainous ranges of Nassau. No diluvial detritus is seen in any part of the duchy, but quartz pebbles in sand occur in the elevated plain between Selters and Nassau: these are supposed to have been torn from the grauwacké by local causes, and to have been deposited prior to the elevation of that formation. The author, reflecting upon the marine fossils on the summits of some of these mountains, infers, that the horizontal strata were formed at the bottom of a sea, and were subsequently elevated; and he is inclined to attribute the origin of the grauwacké to the attrition of the primitive rocks during the period of their elevation.

ROYAL INSTITUTION OF GREAT BRITAIN.

The following is an account of the proceedings at the Royal Institution, on the Friday evening meetings of the members.

Feb. 3.—The history of caoutchouc was given in the lecture-room by Mr. Faraday, and various specimens relating to its chemical nature and its application in producing waterproof

proof fabrics shown. The latter were prepared by Mr. Hancock.

Feb. 10.—The progress made by Mr. Brunell in his application of the condensed carbonic acid to the construction of a mechanical engine was described to the members by Mr. Faraday, and stated to be highly favourable.

Feb. 17.—Mr. Griffiths's experiment on the state of alkali in glass, Mr. Varley's single adjustable microscope, Mr. Brant's large bar of palladium, and a South American Geological series of specimens were shown and explained in the library.

Feb. 24.—Mr. Varley explained the nature of his graphic telescope intended for the use of artists. It combines magnifying powers with the properties of Dr. Wollaston's camera lucida.

Mar. 3.—The art of lithography was illustrated by numerous operations, and its minute chemical and mechanical principles explained by Mr. Faraday, and Mr. Hullmandel, who furnished the beautiful specimens shown.

Mar. 10.—Mr. Brande entered into the chemical history of wines as respected the alcohol contained in them; and showed the state of combination in which it was retained, the consequent loss of part of its power, and the most perfect modes of analysis. Some specimens of unadulterated port and very old hock were operated upon.

ROYAL ACADEMY OF SCIENCES OF PARIS.

Nov. 7, 1825.—A letter from M. de Gregori was read, relative to the success of vaccination in the Piedmontese states.—M. D'Hombre-Firmas communicated a memoir on a great depression of the barometer observed at Alais in October last.—Dr. Rouzé presented a memoir in manuscript, entitled, An explanation of the famous problem of general electricity.—Dr. Candiloro, of Palermo, presented a memoir, entitled, Medico-chirurgical reflections on the quickest and surest means of extracting calculi from the bladder.—M. Latreille was appointed to make a verbal report on M. de Blainville's work, entitled, "Manuel de Malacologie et de Conchyologie.—M. Dupuytren read the second part of the report of the committee appointed to examine the memoirs on the yellow fever and on the plague.—M. de Ferussac read a memoir, entitled, A methodical table of the class of *Cephalopoda*, presenting a new classification, by M. Dessalines d'Orbigny, jun.

Nov. 14.—M. Paul Laurens communicated a memoir on aërial perspective.—M. Lejeune d'Irichlet communicated a supplement to his memoir on the impossibility of some indeterminate equations of the fifth degree.—M. Amussat communicated a memoir on the different rights of priority in the discovery of lithontriptic methods.—M. Rœstrentret communicated a plan of an instrument for sounding at the greatest depths.—M. Magendie, in the name of Mr. Hulkens, a clockmaker at Philadelphia, presented an improved instrument for executing the same operations as those of MM. Amussat, Civiale, &c.—M. Girard made a report on the machine presented to the King by M. Blanc, of Grenoble.—MM. Geoffroy St. Hilaire, Latreille and Duméril gave a report on M. Serres's work on animal monsters.—M. Duméril gave a verbal account of M. de Blainville's comparative anatomy.—M. de la Billardière read a report on M. Poiret's History of the Plants of Europe.

Nov. 21.- M. Libri communicated a memoir, in which he discusses various questions relative to the analytical theory of heat.—M. Dupuytren read the third and last part of the report of the committee on the memoirs on yellow fever, &c.

XXXVI. *Intelligence and Miscellaneous Articles.*

COMET.

ANOTHER comet has been discovered this year, by Captain Biela, at Josephstadt. It was first seen in Æ 27° 38′, and N. decl. 9 47′: but both its right ascension and declination were diminishing.

THE PANTOCHRONOMETER.

In vol. lxiii. of the Philosophical Magazine we noticed Essex's Portable Damp Detector, an useful application of hygrometry to the purposes of good housewifery and the preservation of health. The same ingenious artist has produced an instrument called the Pantochronometer, intended, by a neat combined application of several principles of nature and facts in astronomy, to instruct young persons in the variation of time according to longitude, in a very amusing manner. A sun-dial is supported by a magnetic needle, adjusted to the variation in the different longitudes for which the instrument is constructed, and the divisions of the hours on which are made to indicate, in an outer fixed circle, the corresponding time at most places of consequence on the globe. The principle and applications of the Pantochronometer are perspicuously explained in a work which is sold with it; and, altogether, we think the invention a very useful addition to our stock of means for imparting scientific knowledge to the juvenile mind.

CHEMICAL OBSERVATIONS: BY MR. MURRAY.

1. *Singular Modification of Temperature by Copper and Silver Leaf.*

When we grasp in the hand a few foils of copper or silver leaf, a peculiar glow of temperature is communicated and felt. I found that a delicate thermometer placed in the hollow of the hand, the ball completely enveloped, indicated 98° 5′ F.; with the ball wrapped round with loose copper leaf, the temperature shown was 101° F.; with silver leaf, 101°+F.; with mixed silver and copper foil, 99° 75′.

2. *Aphlogistic Phænomena of Gum.*

If a portion of powdered gum arabic be placed on a disc of platinum and burnt to charcoal, it will, when ignited, continue long to glow in that state: let fall on paper it ignites the paper; and placed on the platinum wire of the "lamp without flame" it continues aphlogistic with the coils, and it will ignite a sulphur match, &c.

The platinum cage supplied with these aphlogistic live coals produces a fine effect; and when even the platinum has become extinct, they will continue to glow and give light, and the reignition of the wick is more certainly secured.

When the aphlogistic gum is introduced into a glass cylinder containing a portion of ether, it will glow in the superior part of the vessel; when immersed too low and brought near the surface of the ether, it will be apparently extinguished; but when raised to its former position it will be rekindled, and continue to glow with additional brightness. There is this difference, however, between the platinum and the gum; there is no appretiable waste in the former, while there is a sensible expenditure in the latter.

When this substance reduced nearly to whiteness is introduced into the lower part of the flame of coal gas, the starlike brilliancy is excessive. It will be easy now to account for the peculiar intensity of light which supervenes on introducing into the exterior verge of common flame, platinum wire, bits of straw, &c. reduced to whiteness,—the brilliancy of magnesia, &c. before the ignited gas in the compound blowpipe, &c.—these being all reducible to the class of aphlogistic phænomena.

3. *Liquid Aqueous Ammonia, burning with Flame in Chlorine.*

It is well known that if two cylinders, the one containing chlorine and the other ammoniacal gas, be brought in contact, a flash of light pervades the interior. Professor Silliman has also stated that a large volume of ammoniacal gas may be ignited and continue to burn; and Dr. Henry has adverted to a phænomenon of the same description: indeed, the foreign flame exhibited when a lighted taper is introduced into a small cylinder of this gas, sufficiently proves that the gas is inflammable in atmospheric air. I find, however, that if a slip of paper be dipped in strong liquid ammonia, and immersed into a cylinder of newly prepared chlorine, it will burn with a beautiful flame, and the liquid ammonia will also burn with flame when introduced in the deflagrating spoon.

FOSSIL REMAINS.

Notwithstanding the confused and unscientific manner in which this account is drawn up, we think there is reason to believe that some interesting fossil remains have been found; and not wishing to assume any responsibility for the correctness of the notice, we give it as originally published. It is to be hoped that a more satisfactory description of these remains will soon be received.

Our enterprising fellow-citizen, Mr. Samuel Schofield, has disinterred from the low prairie grounds between Placquemine and the lakes, a number of remains of the most gigantic size. They evidently belong to some class of animals now no longer in existence; whether antediluvian or not, we are unable to say. The great *Elephas mastodon*, or American Mammoth, described by Dr. Mitchill, is inferior in size to these bones we have seen. From the circumstance of ambergris being collected in some quantity from the inferior surface of the maxillary bone, we are led to the conclusion that they are of marine origin, but of what description we are unable to conjecture. Upon examining these remains, we are easily led to give credit to the extraordinary relations given by Father Kircher, of the Kraken and Norway sea snake. This nondescript, when alive, must have equalled either of them in bulk.

We will attempt a faint description of those which have already been brought up to this city, and are now on board the steam-boat Expedition. They consist, first, of an enormous fragment of a cranium. It is about twenty-two feet in length, and its broadest part four feet high, and perhaps nine inches thick. It is said to weigh about twelve hundred pounds. On the interior surface the vitreous table appears to be separated from the cancelli for some way down; this table is perfectly

firm,

firm, and in a perfect state of preservation; the digital depressions formed by the convolutions of the cerebellum are very perfect.

The foramina for the passage of the sensorial nerves are very discernible. A very large portion of the inner table of the inside of the cranium is joined by a very singular squamous suture. The inner surface appears in many places permanently discoloured by the bed of earth from whence it was taken. In the interior part of the cranium the diploë presents a very singular appearance, the cavities of which are very large, in some cases presenting holes of nearly an inch in diameter, and generally very regular. Upon what we judge to be the temporal portion, a most singular process or elongation presents itself: it is eight feet in length, and of a triangular form, and about six inches through, tapering gradually to the point. This singular appearance sets all our conjectures at defiance; it is of a spongy construction, with a rough and irregular surface. There appears to be no seat for the insertion of muscles, or foramina for the passage of the nerves or blood-vessels.

This bone must have been covered for its whole length with a membrane. The cancelli are remarkably regular. There is a singular consolidation of the nasal and maxillary bones. They are not united by any of the description of sutures found in quadrupeds, but form one entire mass of uniform consistence all through. A large groove or canal presents itself in the superior portion of this bone, upon the side of which considerable quantities of ambergris may be collected, which appears to have suffered little or no decomposition or change by age. It burns with a beautiful bright flame, and emits an odoriferous smell while burning; it is of a greasy consistence, similar to adipocire.

The foramen for the transmission of the facial nerve is of an immense size.

In the inferior portion of this stupendous bone there appears to be an articulating depression, in which the superior angle of the lower jaw might have been articulated.

The other bones are; one of a cylindrical shape, with a round head similar to the os humeri in quadrupeds. It is two feet in length, and about ten inches in diameter, with about two processes near the head, in some respects similar to the trochanters of the femoris. The cartilaginous extremities appear to have been entirely detached. Upon one end a surface for the articulation of two bones appears, one of which is in the collection. This bone is over one foot in length, and of a flattened cylindrical shape; the cartilaginous extremities are also gone. It is of a firmer consistence than any of the other bones, with a singular irradiation of ossific appearance on the outside surface. These two bones are probably the leg of the animal.

There are also lumbar, dorsal, and cervical vertebræ. The cylindrical portions of those of the first class are fourteen inches in diameter, with transverse processes, in every respect like those of quadrupeds. One of them has the introvertebral substance completely detached; it is about twelve inches in diameter, and perhaps two inches thick in the centre, tapering gradually to the extremities; this specimen is in a perfect state of preservation. In the articulation of these bones there is considerable analogy to the human vertebræ.

To judge from the appearance of this portion of the cranium which we have seen, if this monster was of the *Balæna* species, his length could not be less than two hundred and fifty feet. It is stated, that from this place, whence these remains were disinterred, a large carnivorous tooth was found, and has been carried away. It is also related, that in the year 1799, many remains of antediluvian creation were taken up near this same place, and shipped to Europe.—*Boston Journal of Philosophy*, Aug. 1825.

VOLCANO IN OWHYHEE.

Mr. William Ellis, a missionary, in his narrative of a tour through the island so well known as the place where Captain Cook was murdered, gives the description of a volcano of a singular kind, of which we shall select for our readers some of the most striking particulars. Mr. Ellis passed over a large tract of volcanic country with burning chasms and hills, which had the appearance of having been craters. The plain over which their way lay, was a vast waste of ancient lava, which he thus describes:

" This tract of lava resembled in appearance an inland sea, bounded by distant mountains. Once it had certainly been in a fluid state, but appeared as if it had become suddenly petrified, or turned into a glassy stone, while its agitated billows were rolling to and fro. Not only were the large swells and hollows distinctly marked, but in many places the surface of these billows was covered by a smaller ripple, like that observed on the surface of the sea at the first springing up of a breeze, or the passing currents of air, which produce what the sailors call a cat's-paw. * * * *

" About two P.M. the crater of Kirauea suddenly burst upon our view. We expected to have seen a mountain with a broad base and rough indented sides, composed of loose slags, or hardened streams of lava, and whose summit would have presented a rugged wall of scoria, forming the rim of a mighty cauldron. But, instead of this, we found ourselves on the edge of a steep precipice, with a vast plain before us, fifteen or sixteen miles in circumference, and sunk from two hundred to four hundred feet below its original level. The surface of this plain was uneven, and strewed over with huge stones and volcanic rock, and in the centre of it was the great crater, at the distance of a mile and a half from the place where we were standing. . . . . We walked on to the north end of the ridge, where, the precipice being less steep, a descent to the plain below seemed practicable. . . With all our care, we did not reach the bottom without falls and slight bruises. . . After walking some distance over the sunken plain, which in several places sounded hollow under our feet, we at length came to the edge of the great crater, where a spectacle sublime, and even appalling, presented itself before us. Immediately before us yawned an immense gulf, in the form of a crescent, about two miles in length, from N.E. to S.W. nearly a mile in width, and apparently eight hundred feet deep. The bottom was covered with lava, and the S.W. and northern parts of it were one vast flood of burning matter, in a state of terrific ebullition, rolling to and fro its 'fiery surge' and flaming billows. Fifty-one conical islands of varied form and size, containing so many craters, rose either round the edge, or from the surface of the burning lake; 22 constantly emitted columns of gray smoke, or pyramids of brilliant flame; and several of these at the same time vomited from their ignited mouths streams of lava which rolled in blazing torrents down their black indented sides, into the boiling mass below. The existence of these conical craters led us to conclude that the boiling cauldron of lava before us did not form the focus of the volcano; that this mass of melted lava was comparatively shallow; and that the basin in which it was contained was separated by a stratum of solid matter from the great volcanic abyss, which constantly poured out its melted contents through these numerous craters into this upper reservoir.

"The sides of the gulf before us, although composed of different strata of ancient lava, were perpendicular for about 400 feet, and rose from a wide horizontal ledge of solid black lava of irregular breadth, but extending completely round: beneath this ledge, the sides sloped gradually towards the burning lake, which was, as nearly as we could judge, three hundred or four hundred feet lower. It was evident that the large crater had been recently filled with liquid lava up to this black ledge, and had, by some subterraneous canal, emptied itself into the sea or under the low land on the shore. The gray, and in some places apparently calcined sides of the great crater before us—the fissures which intersected the surface of the plain on which we were standing—the long banks of sulphur on the opposite side of the abyss—the vigorous action of the numerous small craters on its borders—the dense columns of vapour and smoke that rose at the N. and S. end of the plain —together with the ridge of steep rocks by which it was surrounded, rising probably in some places 300 or 400 feet in perpendicular height, presented an immense volcanic panorama, the effect of which was greatly augmented by the constant roaring of the vast furnaces below."

LIST OF NEW PATENTS.

To James Fraser, of Houndsditch, London, for his improved method of constructing capstans and windlasses.—Dated 25th of February 1826.—2 months allowed to enrol specification.

To Benjamin Newmarch, of Cheltenham, for certain inventions to preserve vessels and other bodies from the dangerous effects of external or internal violence on land or water.—25th of February.—6 months.

To Benjamin Newmarch, of Cheltenham, for a preparation, to be used either in solution or otherwise, for preventing decay in timber, &c. arising from dry rot, &c.—25th of February.—6 months.

To James Fraser, of Houndsditch, London, for his improved method of distilling and rectifying spirits, &c.—4th of March.—2 months.

To Robert Midgley, of Horsforth near Leeds, for his apparatus for conveying persons and goods across rivers or other waters, and over valleys.—4th of March.—6 months.

To George Anderton, of Chickheaton, Yorkshire, worsted spinner, for improvements in the combing or dressing of wool and waste silk.—4th of March.—2 months.

To James Neville, of New Walk, Shad Thames, Surrey, engineer, for his improved boiler for generating steam with less expenditure of fuel.—14th of March.—6 months.

To Nicholas Hegesippe Manicler, of Great Guildford-street, Southwark, chemist, for his new preparation of fatty substances, and the application thereof to the purposes of affording light.—20th of March.—6 months.

Results of a Meteorological Journal for the Year 1825, *kept at the Observatory of the Royal Academy, Gosport, Hants.*

By William Burney, *LL.D.*

Latitude 50° 47′ 20″ North—Longitude 1° 7′ West of Greenwich. In time 4′ 28″.

1825. Months.	Barometer.										Self-registering Thermometer.								Mean Temp. of Spring Water.	De Luc's Hygrometer.						
	Max.	Min.	Media.	Range.	No. of Changes.	Spaces described.	Greatest Variation in 24 hours.	Media at 8 A.M.	Media at 2 P.M.	Media at 8 P.M.	Max.	Min.	Media.	Mean Range.	Gt. Var. in 24 hours	Media at 2 P.M.	Media at 8 A.M.	Media at 8 P.M.		Max.	Min.	Mean Range of the Index.	Media at 2 P.M.	Media at 8 A.M.	Media at 8 P.M.	Media at 8,2,&8 o'cl.
	In.	In.	In.	In.		In.	In.	In.	In.	In.	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°	°
January	30·82	29·35	30·220	1·46	20	7·16	0·57	30·212	30·212	30·230	57	32	42·32	25	23	45·84	40·22	42·35	50·69	92	58	34	69·2	76·4	74·3	73·3
February	30·52	29·48	30·100	1·04	17	6·14	0·57	30·101	30·104	30·107	57	30	43·61	27	19	48·46	41·11	42·86	49·65	84	55	29	67·3	74·2	73·4	71·6
March...	30·57	28·90	30·034	1·67	24	6·47	0·77	30·037	30·037	30·033	59	29	44·18	30	20	49·77	41·68	43·23	48·97	85	49	36	60·5	68·1	68·1	65·6
April ...	30·42	29·27	29·980	1·15	31	3·89	0·40	29·987	29·981	29·972	66	34	52·22	32	27	58·70	51·50	49·33	48·97	81	41	40	55·2	60·7	65·8	60·6
May ...	30·40	29·55	29·966	0·85	24	4·02	0·30	29·967	29·973	29·965	72	41	56·71	31	24	62·74	57·00	53·81	49·50	86	42	44	56·7	61·2	66·7	61·5
June ...	30·44	29·40	30·036	1·04	24	4·61	0·46	30·042	30·033	30·012	82	41	60·80	41	25	68·13	62·20	58·67	50·49	79	42	37	51·0	54·1	60·3	55·2
July ...	30·34	29·83	30·128	0·51	18	3·07	0·34	30·134	30·131	30·122	86	50	65·66	36	24	73·64	66·13	65·39	51·75	67	40	27	48·3	53·7	57·2	53·1
August	30·35	29·30	29·955	1·05	17	4·25	0·60	29·948	29·954	29·943	86	50	64·72	36	27	70·81	64·19	62·90	53·36	96	37	59	58·3	65·9	70·7	64·9
Septemb.	30·33	29·43	29·895	0·90	21	4·19	0·36	29·893	29·898	29·898	78	47	63·62	31	22	69·50	62·53	61·37	54·58	95	45	50	61·5	69·8	76·0	69·1
October	30·55	28·97	29·969	1·56	22	7·27	0·71	29·974	29·979	29·964	71	35	54·74	36	20	58·87	53·71	53·45	55·01	100	53	47	67·6	75·1	81·4	74·7
Novemb.	30·39	28·60	29·722	1·70	24	9·89	0·76	29·736	29·706	29·710	60	29	44·97	31	22	48·77	43·20	45·00	53·84	100	59	41	76·0	82·5	82·2	80·2
Decemb.	30·10	29·00	29·571	1·10	26	6·27	0·60	29·564	29·571	29·582	55	26	42·82	29	16	44·81	40·58	42·35	51·88	98	54	44	82·9	88·8	86·4	86·0
Averages for 1825.	30·82	28·60	29·964	14·03	268	67·23	0·77	29·966	29·965	29·961	86	26	53·01	32·1	27	58·34	52·00	51·72	51·56	100	37	40·7	62·9	69·2	71·9	68·0

1825.	Scale of the Winds.									Modifications of Clouds.							Weather.						Atmospheric Phænomena.									Evaporation in Inches, &c.	Rain in Inches, &c.
	North.	North-East.	East.	South-East.	South.	South-West.	West.	North-West.	Total Number of Days.	Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostratus.	Nimbus.	A clear Sky.	Fair, with Clouds.	An overcast Sky.	Foggy.	Rain, &c.	Total Number of Days.	Anthelion.	Parhelia.	Paraselenæ.	Solar Halos.	Lunar Halos.	Rainbows.	Meteors.	Lightning.	Thunder.		
January ..	7½	3½	½	½	2	4½	6½	6	31	11	6	31	1	12	21	17	4½	12	10½	0	4	31	0	1	0	0	2	0	2	0	0	0·76	1·480
February	1	3½	3	3	4	4½	4	5	28	11	6	25	0	12	18	13	3½	10	9	1½	4	28	0	0	0	1	2	0	2	0	0	1·00	1·210
March ...	3½	11½	2	4½	1	2½	4	2	31	17	6	25	1	12	18	15	4	13	9½	1	3½	31	0	2	0	2	1	0	4	0	0	2·80	2·500
April	2	6	1	6	2½	5	3½	4	30	25	13	25	3	15	9	9	5	18	3	0	4	30	0	0	1	2	2	0	4	0	0	4·48	1·890
May	3	7	4	4½	3	4	3	2½	31	21	21	29	1	25	27	18	4	17	4½	0	5½	31	0	0	0	4	0	2	6	6	2	4·90	2·995
June	2½	5½	½	2	2	10	4½	3	30	20	17	25	0	24	16	15	4	18	4½	0	3½	30	0	2	0	2	0	1	7	1	1	7·60	1·575
July	4½	10	2½	4	2	2	3½	2½	31	19	13	23	2	23	17	5	12½	15	2½	0	1	31	0	0	0	0	0	0	16	1	0	10·37	0·180
August ..	1½	5	1	2½	1	10½	4½	5	31	17	15	27	2	25	25	17	4	13½	8	½	5	31	0	0	0	1	0	1	40	3	2	5·05	3·115
September	4½	2	4	1½	3½	8½	1½	4½	30	24	13	25	3	24	25	17	3½	15½	6½	0	4½	30	0	3	1	2	2	2	24	3	1	4·60	2·430
October ..	2½	0	½	2	6	5	5	10	31	21	12	31	1	20	25	18	3½	11½	10	0	6	31	0	0	0	0	0	0	38	1	0	2·55	3·025
November	3½	1	1	½	1	6	9	8	30	13	10	26	1	12	21	24	4	10	8½	0	7½	30	0	0	0	0	2	2	14	1	1	1·40	4·725
December	1	4	1	5	2	4	5	9	31	17	10	30	4	9	25	24	3	11½	8½	1	7	31	0	0	0	1	4	1	2	3	0	1·10	5·325
Averages for 1825.	37	59	21	36	30	66½	54	61½	365	216	142	322	19	213	247	192	55½	165	85	4	55½	365	0	8	2	15	15	9	159	19	7	46·61	30·450

ANNUAL RESULTS FOR 1825.

Barometer.	Inches.
Greatest pressure of the atmosphere, Jan. 9th, Wind N.	30·820
Least ditto ditto Nov. 10th, Wind N.E.	28·600
Range of the mercury	2·220
Annual mean pressure of the atmosphere	29·964
Mean pressure for 177 days, with the moon in North declination	29·983
Mean pressure for 177 days, with the moon in South declination	29·968
Annual mean pressure at 8 o'clock A.M.	29·966
—— at 2 o'clock P.M.	29·965
—— at 8 o'clock P.M.	29·961
Greatest range of the mercury in November	1·700
Least range of ditto in July	0·510
Greatest annual variation in 24 hours in March	0·770
Least of the greatest variations in 24 hours in May	0·300
Aggregate of the spaces described by the rising and falling of the mercury	67·230
Number of changes	268·

Self-registering Day and Night Thermometer.	
Greatest thermometrical heat, July 19th, Wind S.E.	86½°
—— cold, Dec. 27th and 30th, Wind N. W.	26
Range of thermometer between the extremes	60½
Annual mean temperature of the external air	53·01
—— of do. at 8 A.M.	52·00
—— of do. at 8 P.M.	51·72
—— of do. at 2 P.M.	58·34
Greatest range in June	41·00
Least of the monthly ranges in December	29·00
Annual mean range	32·10
Greatest monthly variation in 24 hours in April and August	27·00
Least of the greatest variations in 24 hours in December	16·00
Annual mean temperature of spring water at 8 A.M.	51·56

De Luc's *Whalebone Hygrometer.*	Degrees.
Greatest humidity of the atmosphere, October 12th and November 10th.	100
Greatest dryness of ditto, August 1st	37
Range of the index between the extremes	63
Annual mean of the hygrometer at 8 o'clock A.M.	69·2
—— at 8 o'clock P.M.	71·9
—— at 2 o'clock P.M.	62·9

Annual

	Degrees.
Annual mean of the hygrometer at 8, 2, & 8 o'clock	68·0
Greatest mean monthly humidity of the atmosphere in December	86·0
Greatest mean monthly dryness of ditto in July	53·1

Position of the Winds.

	Days.
From North to North-east	37
—— North-east to East	59
—— East to South-east	21
—— South-east to South	36
—— South to South-west	30
—— South-west to West	66½
—— West to North-west	54
—— North-west to North	61½
	——365

Clouds, agreeably to the Nomenclature; or the Number of Days on which each Modification has appeared.

	Days.
Cirrus	216
Cirrocumulus	142
Cirrostratus	322
Stratus	19
Cumulus	213
Cumulostratus	247
Nimbus	192

General State of the Weather.

	Days.
A transparent atmosphere without clouds	55½
Fair, with various modifications of clouds	165
An overcast sky, without rain	85
Foggy	4
Rain, hail, and sleet	55½
	——365

Atmospheric Phænomena.

	No.
Parhelia, or mock-suns, on the sides of the true sun	8
Paraselenæ, or mock-moons	2
Solar halos	15
Lunar halos	15
Rainbows, solar and lunar	9
Meteors of various sizes	159
Lightning, days on which it happened	19
Thunder, ditto ditto	7

Evaporation.

	Inches.
Greatest monthly quantity in July	10·37

Least

Least monthly quantity in January	0·76 In.
Total amount for the year	46.61

Rain.

Greatest monthly depth in December . . .	5·325
Least monthly depth in July	0·180
Total depth near the ground for the year .	30·450
Total depth 23 feet high, for ditto	27·200

N. B. The barometer is hung up in the observatory 50 feet above the low-water mark of Portsmouth Harbour; and the self-registering horizontal day and night thermometer, and De Luc's whalebone hygrometer, are placed in open-worked cases, in a northern aspect, out of the rays of the sun, 10 feet above the garden ground. The pluviameter and evaporator have respectively the same square area: the former is emptied every morning at 8 o'clock, after rain, into a cylindrical glass gauge accurately graduated to 1-100th of an inch; and the quantity lost by evaporation from the latter, is ascertained at least every third day, and sometimes oftener, when great evaporations happen by means of a high temperature, and dry northerly or easterly winds.

Barometrical Pressure.—In consequence of the high pressure of the atmosphere during the first three months, also in June and July, the mean height of the barometer is greater this year by 79-1000th of an inch, than the mean of the last eleven years. This was the case in every part of the country with some little differences. The aggregate of the spaces described by the alternate rising and falling of the mercury is 15·89 inches less this year than last, and the number of changes seven less, which indicate a comparatively uniform pressure.

For 177 days in which the moon ranged in North declination, the pressure was 3-200ths of an inch greater than that in the 177 days in which she ranged in South declination.

Temperature.—The annual mean temperature of the external air is exactly one degree higher than that in 1824, and 1·39 degree higher than the mean of the last ten years. The mean temperature of June, July, August and September was high, and these months were dry, particularly July, when we experienced oppressive heat for several days: but the spring and autumn were rather cold, which in great measure equalized the annual average temperature of 1824 and 1825.

The annual mean temperature of spring water at 8 o'clock A.M. this year, is nearly a degree and a half lower than the annual mean temperature of the external air.

Wind.—The crossing and opposite winds, or upper currents, have been found to prevail very much this year.

In comparing the Scale of the Winds in 1824 and 1825, there appears a near coincidence in their duration from six out of eight points of the compass; but there is a great difference in the North-east and South-west winds this year; the former having prevailed longer by nearly one-third, and the latter a less time by nearly one-fifth. The longer duration of the North-east wind, with the additional mean temperature of the atmosphere, seems to accord with the increased evaporation, which is nearly one-third more this year than last; and the shorter duration of the South-west wind, was the means of keeping back about one-fourth of the comparative depth of rain: besides, the gales from the South-west have not been so prevalent as they were last year. Such is the influence the winds appear to have in drying and condensing the lower stratum of air, in connexion with the temperature of the ground.

The following is the number of strong gales of wind, or days on which they have prevailed, this year:

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Gales.
1	10	1	5	5	26	5	4	57

The gales from the S.W. are more than half the number in the scale.

Clouds.—The following is a correct scale of the clouds agreeably to the nomenclature, being the number of days on which each modification has appeared during the last *nine years*, ending with 1825.

Cirrus.	Cirro-cumulus.	Cirro-stratus.	Stratus.	Cumulus	Cumulo-stratus.	Nimbus.
1838	1476	2582	295	1711	1708	1773

By this scale the *cirrostratus* appears to be the prevailing cloud, having appeared more than three-fourths of this long period.

The *cumuli* and *cumulostrati* are nearest in the times of their appearance; and the *cirri* and detached *nimbi* the next nearest. The *cirrocumuli* and *strata* are the least in number, being in general fair weather clouds.

Weather.—The general state of the weather throughout the year, was calm and dry, but very variable in temperature at intervals; the dry part was in the winter and summer months, and the wet part in spring and autumn. The summer was uniformly hot, which brought on an early corn harvest.

January, March, June, October, and November, were rather windy months, the others comparatively calm.

The spring and summer seasons were healthy, but the winter and autumn were sickly, in consequence of the sudden changes that occurred in the temperature and quality of the air; as it must be acknowledged that health, or sickness, and also the spirits of the human mind, are materially influenced by the good or bad state of the air we inhale, and the means employed to keep the body of an uniform temperature throughout the vicissitudes of the seasons in the variable climate of England and her united kingdoms.

Results of a Meteorological Journal for February 1826, *kept at the Observatory of the Royal Academy, Gosport, Hants.*

General Observations.

This month has been mild for the season, but generally windy and wet, agreeing with the old proverb "February fill dike." It has rained, more or less, on 20 days, and the thermometer a few feet from the ground did not recede once to the freezing point. In consequence of the constant humid air, very little evaporation, and the quantity of rain, the ground was saturated nearly the whole month, and is now in good condition for an early produce of the approaching spring.

The average temperature of the external air this month, is 2⅓ degrees higher than in February 1825, and nearly 3¾ degrees higher than the average of that month for the last ten years. There is a difference in the mean temperature between last month and this of 10⅓ degrees!

The temperature of spring water has increased upwards of one degree this month, and is 1⅕ degree higher than at this time last year. This is certainly an unusual circumstance in February, as the temperature of spring water almost invariably decreases till the vernal equinox, and sometimes later. The last two or three days having been dry, and the temperature of the ground increasing, there was therefore a sudden appearance of the fruit and other trees breaking into bud.

Although the wind has prevailed half the month from the S.W. and W., yet the result of the barometer is above the general mean indication, arising no doubt from the closer union of the atmospherical particles, and a lower temperature in the superior stratum of air not far above the disturbing force of the late S.W. gales of wind.

The atmospheric and meteoric phænomena that have come within our observations this month, are, one parhelion, one solar and one lunar halo, three meteors, and eight gales of wind, or days on which they have prevailed; namely, one from S.E. and seven from S.W.

Numerical Results for the Month.

Inches.
Barometer { Maximum 30·44, February 26th—Wind S.W.
Barometer { Minimum 29·33, Ditto 17th—Wind S.
Range of the mercury . . 1·11.
Inches.
Mean barometrical pressure for the month . . . . . . 29·957
——— for the lunar period ending the 7th inst. . . 30·006
——— for 15 days, with the Moon in North declin. 30·218
——— for 15 days, with the Moon in South declin. 29·794
Spaces described by the rising and falling of the mercury 6·000
Greatest variation in 24 hours . . . . 0·680
Number of changes . . . . 26·
Thermometer { Maximum 56°, February 25th and 28th.
Thermometer { Minimum 33 Ditto 9th—Wind E.
Range 23
Mean temp. of the external air 45·91
——— for 30 days with the Sun in Aquarius . . . . } 43·23
Greatest variation in 24 hours 16·00
Mean temp. of spring water at 8 o'clock A.M. . . . } 49·44

De Luc's *Whalebone Hygrometer.*

Degrees.
Greatest humidity of the air . 96 in the morning of the 6th.
Greatest dryness of ditto . . . 63 aftern. of the 7th & 25th.
Range of the index 33
Mean at 2 o'clock P.M. . . . 75·8
—— at 8 o'clock A.M. . . . 84·4
—— at 8 o'clock P.M. . . . 84·2
—— of three observations each day at 8, 2, and 8 o'clock } 81·5
Evaporation for the month . . . . 1·30 inch.
Rain in the pluviameter near the ground . 3·86
Rain in ditto 23 feet high . . . . 3·42
Prevailing winds, S.W.

Summary of the Weather.

A clear sky, 3; fine, with various modifications of clouds, 11; an overcast sky without rain, 6; foggy, ½; rain, 7½.—Total 28 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
18	9	27	0	17	18	21

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
0	0	2	4	5½	10½	4	2	28

A METERORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr.* Burney *at Gosport, Mr.* J. Cary *in London, and Mr.* Veall *at Boston.*

Gosport, at half-past Eight o'Clock, A.M. Days of Month, 1826.	Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Evaporation.	Rain near the Ground	Clouds. Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Height of Barometer, in Inches, &c. Lond. 1 P.M.	Bost. 8½ A.M.	Thermometer. London. 8 A.M.	Noon.	11 P.M.	Boston. 8½ A.M.	Rain. London.	Boston.	Weather. London.	Boston.	Wind.
☾ Feb. 1	29 77	42	49·25	92	E.	...	[illegible]	...	...	1	...	...	...	1	29·85	29·60	43	45	43	43	...	...	Rain	Cloudy—rain *p.m.*	S.
2	29·81	47	......	86	SW.	...	[illegible]	1	1	1	...	1	1	...	29·88	29·55	45	50	44	45	...	·05	Fair	Cloudy	S.
3	29 68	48	......	81	S.	0·10	·175	...	1	1	...	...	1	1	29·74	29·33	41	49	45	49·5	...	...	Rain	Cloudy	S.
4	29 83	42	......	87	S.	...	·005	...	...	1	...	...	...	1	29·96	29·60	43	48	48	39	...	·20	Fair	Fine—rain *a.m.*	W.
5	29 83	42	......	90	S.	...	·030	1	...	1	...	1	1	1	29 93	29 54	43	44	45	41	...	...	Fair	Fine	S.
6	29 55	51	......	96	SW.	·10	·065	...	...	1	...	...	...	1	29·60	29 10	49	51	48	53	...	·18	Rain	Cloudy & stormy—	SW.
7	30·02	44	......	80	W.	...	......	...	...	1	...	1	...	...	30·15	29 55	42	48	36	45	...	...	Fair	Fine [rain *a.m.*	W.
8	30·30	39	......	87	SW.	...	......	1	...	1	...	1	...	...	30·40	30 03	35	46	38	35·5	...	...	Fair	Fine	SW.
9	30·22	39	49·20	90	SE.	·15	·010	...	...	...	...	1	...	1	30 30	30·00	37	41	31	35·5	...	...	Foggy	Fine	S.
10	30·16	39	......	83	E.	...	......	1	1	1	...	1	1	...	30 27	30·05	30	36	34	36·5	...	...	Fair	Foggy	calm
11	30·10	41	......	81	SE.	...	......	1	1	1	...	1	1	...	30·18	29 90	35	37	38	35	...	...	Fair	Cloudy	S.
12	30·03	46	......	85	S.	·10	·060	...	...	1	...	...	1	1	30·17	29·82	45	48	40	40	...	...	Cloudy	Cloudy	S.
13	30 12	42	......	89	S.	...	·240	1	...	1	...	1	...	1	30 67	29·87	42	[illegible]	42	34	...	[illegible]	Fair	Foggy	calm
14	29·90	45	......	86	S.	...	·165	...	...	1	...	...	...	1	29 97	29·65	[illegible]	46	44	44	...	[illegible]	Rain	Rain	SW.
☽ 15	29·89	47	49·15	82	S.	·20	·495	...	1	1	...	...	1	1	29·92	29·60	45	48	45	40	...	[illegible]	Fair	Rain	calm
16	29·70	47	......	84	S.	...	·385	1	...	1	...	1	1	1	29·75	29·45	45	49	42	43·5	...	[illegible]	Fair	Rain	SE.
17	29·33	48	......	81	S.	...	·200	1	...	1	...	1	1	1	29·42	[illegible]	[illegible]	47	38	47	...	[illegible]	Rain	Stormy	SW.
18	29·67	41	......	82	SW.	·15	·200	1	...	1	...	...	1	1	29 87	29·45	39	45	38	37	...	...	Showers	Fine	S.
19	29·55	46	......	86	SW.	...	·120	...	1	1	...	1	1	1	29·60	29 13	45	50	50	44	...	·3	Cloudy	Stormy—rain *a.m.*	SW.
20	[illegible]	45	49·30	79	W.	...	·170	1	...	1	...	1	1	1	30 06	29·38	43	50	43	45	...	...	Fair	Stormy	W.
21	[illegible]	42	......	85	W.	·20	·390	1	...	1	...	...	1	1	30 36	29 92	38	48	47	40·5	...	...	Fair	Fine	calm
○ 22	[illegible]	50	......	86	SW.	...	·210	1	1	1	...	1	1	1	29·98	29·90	47	50	47	45	...	...	Rain	Cloudy	calm
23	29·90	46	......	85	W.	...	·220	1	...	1	...	...	1	1	29·90	29·53	43	46	37	44	...	...	Rain	Cloudy	S.
24	30·14	36	......	80	NW.	·10	·085	1	1	1	...	1	...	1	30·20	29·78	35	46	47	38	...	...	Fair	Fine	W.
25	30·02	50	......	79	W.	...	......	1	1	1	...	1	1	...	30·10	29·63	49	52	44	49	...	·45	Fair	Cloudy—rain *a.m.*	W.
26	30·40	44	......	80	W.	...	......	1	...	1	...	1	...	...	30·46	29·95	43	46	42	41·5	...	...	Fair	Fine	W.
27	30·34	44	......	82	SW.	...	......	1	...	1	...	1	1	1	30·20	29·85	44	49	45	45	...	...	Fair	Fine	W.
28	30·21	47	50·30	80	W.	·20	·020	1	...	1	...	...	1	1	30·26	29·80	44	54	46	48	...	...	Cloudy	Cloudy	SW.
Average.	29·950	44·28	49·44	[illegible]		1·30	3·860	18	9	[illegible]		17	18	21	30·05	29 64	42	47	42	43·3					

THE

PHILOSOPHICAL MAGAZINE
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XXXVIII. *On the Properties of a Line of shortest Distance traced on the Surface of an oblate Spheroid.* By J. Ivory, *Esq. M.A. F.R.S.*[*]

My intention in treating of the geodetical problem inserted in this Journal for July 1824, p. 35, was to show that by giving a proper form to the coordinates of the surface of the spheroid, the usual analysis might be much shortened, and more simple formulæ of solution obtained. If the polar semi-axis be unit, and $\sqrt{1 + e^2}$ represent the radius of the equator, the equation of the surface will be,

$$\frac{x^2 + y^2}{1 + e^2} + z^2 = 1,$$

z being perpendicular, and x and y parallel, to the equator. Now this equation is satisfied by assuming

$$x = \cos\phi \cos\psi \sqrt{1 + e^2}$$
$$y = \sin\phi \cos\psi \sqrt{1 + e^2}$$
$$z = \sin\psi,$$

the angles ϕ and ψ remaining indeterminate. The coordinates belong to a spherical surface when $e = 0$; and this manner of expressing them, which I have used on other occasions, is well adapted for simplifying the investigation of such properties of the elliptical spheroid as are analogous to those of the sphere.

With regard to the arcs ϕ and ψ, it is obvious that $\sin\psi$ is the distance from the equator (estimated in parts of the polar semi-axis) of a parallel to the equator drawn through the point on the surface of the spheroid; and hence it is obvious that ϕ is the angular distance between the meridian passing through the same point and a given meridian. The arc ψ is the latitude of a parallel to the equator on the surface of the sphere inscribed in the spheroid; but it is not the true latitude of the same parallel on the surface of the spheroid, as I have inadvertently called it in the communication alluded to. I have already noticed the inadvertency in this Journal for April 1825; and have shown that the accuracy of the solution is not affected by it, because the import of the symbol is independent of the name given to it, being fixed by the assumed form of the coordinates. The relation of the arc ψ to the true latitude may likewise be deduced directly from the equation of the surface, or from the expressions of the coordinates, without recurring to particular properties of the spheroid, in the manner following.

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[*] Communicated by the Author.

From a point in the spheroid, of which the coordinates are x, y, z, let a perpendicular p be drawn to the surface, and extended outward to a point of which the coordinates are a, b, c; then,

$$p^2 = (a-x)^2 + (b-y)^2 + (c-z)^2:$$

and if x, y, z vary in the surface of the spheroid, the condition of perpendicularity will be,

$$0 = (a-x)\,dx + (b-y)\,dy + (c-z)\,dz.$$

If the arc u denote the inclination of p to the equator, then, [illegible] and z being perpendicular to that plane, we shall have $c - z = p \sin u$. Also $p \cos u$ will be the projection of p upon the same plane; and, as the spheroid is a solid of revolution, p and its projection will be contained in the meridian which makes an angle φ with the given meridian to which y is perpendicular and x parallel: hence $a - x = p \cos u \cos \varphi$, $b - y = p \cos u \sin \varphi$. The foregoing equation will therefore become by substitution,

$$0 = \cos u\,(dx \cos\varphi + dy \sin\varphi) + dz \sin u.$$

Now substitute the differentials of the coordinates, then the arc φ will disappear; and, having divided by $\cos u \cos \psi\, d\psi$, we shall get

$$\tan\psi = \frac{\tan u}{\sqrt{1+e^2}}.$$

This very simple equation expresses the relation between the arcs ψ and u, of which the latter is the true latitude of the point on the surface of the spheroid. It is usual to call the arc ψ the reduced latitude; but, as this name is purely arbitrary, it seems preferable to define the same arc from some of its geometrical properties. This may be done by saying that u and ψ are the latitudes of the same parallel to the equator, the one on the surface of the spheroid, the other on the inscribed sphere. To the formula already given we may add the two following resulting from it, which are of continual use, viz.

$$\sin\psi = \frac{\sin u}{\sqrt{1+e^2\cos^2 u}},$$

$$\cos\psi = \frac{\cos u\,\sqrt{1+e^2}}{\sqrt{1+e^2\cos^2 u}}.$$

Having now ascertained the import of the arcs φ and ψ, all the properties of the geodetical line are readily deduced from the formulæ investigated in this Journal for July 1824. Using the arc ψ to denote the latitude (on the surface of the inscribed sphere) of a plane parallel to the equator which cuts the geodetical line, put μ' for the azimuth at the point of section; then the most distinguishing property is expressed by this equation, viz.

$$\cos\psi' \sin\mu' = \cos i \qquad (a)$$

where $\cos i$ expresses a quantity which is constantly the same for every point of the geodetical line. If we suppose that the parallel plane moves towards the equator, and finally coincides with it, the foregoing equation will become, $\sin\mu' = \cos i$; whence we learn that the arc i is the inclination of the geodetical line to the equator where it crosses that circle. Conceive a great circle on the surface of the inscribed sphere, which is inclined to the equator in the same angle i; and it readily follows from the rules of spherical trigonometry, that the equation (a) is common to the geodetical line on the surface of the spheroid, and the oblique circle on the surface of the sphere; that is, the two lines have the same azimuth at any two points in the same parallel to the equator. It follows that a parallel which meets the oblique great circle, will likewise meet the geodetical line; and consequently they are both contained within the same limits on either side of the equator. They both extend from the equator to two parallels of which the latitudes, on the inscribed sphere, are $\pm i$; and having touched these planes, they bend back in an opposite direction.

In order to compare the two lines further, it is requisite to fix two initial points, or two points of departure from which to reckon. Having assumed any point in the geodetical line, draw through it a parallel to the equator. This parallel will likewise meet the oblique great circle; and we may suppose the plane of the circle turned about the centre of the sphere, till the point in the parallel comes to the meridian of the point assumed in the geodetical line. These two points, one in the oblique great circle, and one in the geodetical line, are the two initial points required; they are in the same parallel to the equator, and they have the same longitude. If λ and l denote the two latitudes of the parallel to the equator, the first on the sphere, and the other on the spheroid, we shall have, according to the equation before found,

$$\tan\lambda = \frac{\tan l}{\sqrt{1+e^2}}.$$

Suppose now that any other parallel to the equator cuts the

two

two lines: let ψ be the latitude of the parallel on the sphere; s' the arc of the oblique great circle between the parallel and the initial point; and ϕ' the spherical angle subtended by s' at the pole of the sphere, or the difference of longitude between the extremities of s'; also let μ be the azimuth of the oblique circle at the initial point, and μ' the azimuth at the other extremity of s': then we shall have the following equations, viz.

$$\begin{aligned} \cos i &= \cos\lambda \sin\mu \\ d s' &= \frac{d\psi \cos\psi}{\sqrt{\sin^2 i - \sin^2\psi}} \\ d\phi' &= \frac{\cos i \, d\psi}{\cos\psi \sqrt{\sin^2 i - \sin^2\psi}} \\ d\mu' &= \frac{\cos i \, d\psi \sin\psi}{\cos\psi \sqrt{\sin^2 i - \sin^2\psi}}. \end{aligned} \qquad (B)$$

These formulæ express the relations between the differentials of the latitude, longitude, and azimuth of a variable point in a great circle of the sphere having the inclination i to the equator; and their use is to compare them with the like quantities in the geodetical line. The expressions of $d s'$ and $d\phi'$ are the same with those marked (B) in the communication inserted in this Journal for July 1824, except that I have here written $\sin^2 i - \sin^2\psi$ for the equivalent quantity $\cos^2\psi - \cos^2 i = \cos^2\psi - \cos^2\lambda \sin^2\mu$. The expression of $d\mu'$, which is common both to the circle and the geodetical line, has now been added.

Again, let s denote the part of the geodetical line cut off by the same parallel to the equator; and put φ for the difference of longitude of the two extremities of s; that is, for the angle contained between the meridian which passes through the variable extremity of s and the fixed meridian of the two initial points. Then, according to the formulæ marked (A) in the communication alluded to, we shall have,

$$\begin{aligned} d s &= d s' \times \sqrt{1 + e^2 \sin^2\psi}, \\ d\varphi &= d\phi' \times \frac{\sqrt{1 + e^2 \sin^2\psi}}{\sqrt{1 + e^2}}. \end{aligned} \qquad (A)$$

These different formulæ, extremely simple, contain all that is necessary to a complete theory of a geodetical line on the surface of an oblate spheroid. I have here merely supplied the geometrical explanation of the analytical solution before given.

Let us compare the longitude in the geodetical line with that in the oblique great circle. The second formula (A) shows that $d\phi$ is always less than $d\varphi'$; and hence the longitude in the geodetical line continually falls behind the longitude in the great circle, the defect accumulating more the further the line is produced. If therefore we suppose that the variable parallel to the equator begins to move from the two initial points, passes beyond the equator to the extreme latitude $-i$, then returns to the other extreme latitude $+i$, and lastly, falls down to the situation it first left; the moveable point in the oblique great circle will have made a complete circle in longitude, and will have returned to its original place; but the moveable point in the geodetical line, although it will have returned to the same latitude it left, will not have completed a circle of longitude, and therefore it will meet the parallel of latitude in a point different from its first place. Thus a geodetical line does not return into itself; if it be continued for several successive circuits of the spheroid, it will form a spiral line upon its surface. It is manifest from the analytical expression of $d\phi$, that when i, which is the limit of ψ, is very small, the whole arc of longitude answering to one turn of the geodetical line, approaches very nearly to $\frac{360^\circ}{\sqrt{1+e^2}}$; or, if we estimate the longitudes on the equator of the spheroid, it will be equal to an arc of the same length with the periphery of the inscribed sphere. Accurately speaking, the arc mentioned is the limit of the longitude made in one turn, when the geodetical line cuts the equator in an indefinitely small angle. It follows therefore that the equator itself is not comprehended in the analytical expression of the arcs of shortest distance; but, when the inclination to the equator is infinitely small, all the turns of the spiral curve become blended with one another and with the equator.

We may next compare the lengths of the geodetical line with the arcs of the oblique great circle cut off by the same parallel to the equator. The first formula (A) shows that ds is greater than ds'; and hence an entire turn of the geodetical line is greater than the periphery of the great circle. But these two quantities approach nearer to an equality as the obliquity to the equator increases; so that they are exactly equal when the geodetical line makes an infinitely small angle with the equator. This agrees with what has already been said respecting the longitude in the same circumstances.

From the same expression it follows that all the turns of the same geodetical line are equal and similar; and even that every single turn consists of four equal parts or quadrants: for the integral of ds has the same value, while ψ varies between the limits 0 and $\pm i$.

Let a denote the arc of the oblique great circle between the initial point and the equator; then $a - s'$ will be the arc between the equator and the variable parallel; and we shall have these equations,

$$\sin \lambda = \sin i \sin a$$

$$\sin \psi = \sin i \sin (a - s').$$

Now substitute this value of $\sin \psi$, then

$$ds = ds' \sqrt{1 + e^2 \sin^2 i \sin^2 (a - s')};$$

and this formula shows that the lengths of a geodetical line, reckoned from a fixt point on the surface of the spheroid, are equal to the arcs of an ellipse reckoned from a fixt point in the periphery. The greater semi-axis of the ellipse is equal to $\sqrt{1 + e^2 \sin^2 i}$, which is the semi-diameter of the spheroid perpendicular to the plane of the oblique great circle; the less semi-axis is equal to the radius of the inscribed sphere.

Enough has now been said to show the use of the formulæ in investigating the properties of a geodetical line. There can be no difficulty in this respect, at least if we suppose that the figure of the spheroid, or the proportion of the polar axis to equatorial diameter, is known. Without knowing this proportion, we cannot deduce the inclination of the oblique great circle to the equator, nor pass from the latitudes actually observed on the surface of the spheroid, to the corresponding latitudes on the surface of the sphere. In the question of the figure of the earth, the problem must therefore be viewed a little differently. It is necessary to introduce the angles actually found by observation in the expression of the length of the geodetical line. Now, we have,

$$\cos i = \cos \lambda \sin \mu;$$

and, by substituting the value of $\cos \lambda$, we get

$$\cos i = \frac{\cos l \sin \mu \sqrt{1 + e^2}}{\sqrt{1 + e^2 \cos^2 l}}$$

$$\sin i = \frac{\sqrt{1 - \cos^2 l \sin^2 \mu + e^2 \cos^2 l \cos^2 \mu}}{\sqrt{1 - e^2 \cos^2 l}}.$$

Let us now put

$$\cos \beta = \cos l \sin \mu:$$

then,

$$\sin i = \frac{\sqrt{\sin^2 \beta + e^2 \cos^2 l \cos^2 \mu}}{\sqrt{1 + e^2 \cos^2 l}}.$$

The arc β being deduced from the latitude and azimuth on the spheroid, is always known; and it is very little different from i.

Again: if we combine the values of ds and ds', found in the formulæ (A) and (B), we shall get,

$$ds = \frac{d\psi \cos \psi \sqrt{1 + e^2 \sin^2 \psi}}{\sqrt{\sin^2 i - \sin^2 \psi}}.$$

But, u and ψ being the latitudes of the same parallel to the equator on the spheroid and the sphere, we have,

$$\sin \psi = \frac{\sin u}{\sqrt{1 + e^2 \cos^2 u}} :$$

and if we transform the expression of ds, by introducing the values of $\sin \psi$ and $\sin i$, we shall obtain,

$$\Delta = \sqrt{\sin^2 \beta + e^2 \cos^2 l \cos^2 \mu - \sin^2 u \, (1 + e^2 \cos^2 l \cos^2 \mu)},$$

$$ds = \frac{(1+e^2) \sqrt{1 + e^2 \cos^2 l} \, . \, du \cos u}{(1 + e^2 \cos^2 u)^{\frac{3}{2}} \, . \, \Delta} . \qquad (C)$$

This formula determines the length of s by means of the latitudes at the two extremities, and the initial direction with respect to the meridian.

In the first place, if the geodetical line be in the direction of the meridian, then $\sin \mu = 0$, $\cos \mu = 1$, $\sin \beta = 1$: hence

$$\Delta = \cos u \sqrt{1 + e^2 \cos^2 l},$$

$$ds = \frac{(1+e^2) \, du}{(1 + e^2 \cos^2 u)^{\frac{3}{2}}} .$$

If we expand the radical and integrate as usual between the limits l and u, supposing u greater than l, we shall get,

$$\frac{s}{P} = u - l + e^2 \left\{ \frac{u-l}{4} - \frac{3}{8} (\sin 2u - \sin 2l) \right\}$$
$$- e^4 \left\{ \frac{3(u-l)}{64} - \frac{3(\sin 2u - \sin 2l)}{32} - \frac{15(\sin 4u - \sin 4l)}{256} \right\} .$$

I have here written $\frac{s}{P}$ for s, on the supposition that s is the actual length measured, and P the semi-polar axis expressed in the same parts. There are therefore two unknown quantities P and e^2; and consequently two different measurements are required at different latitudes. The latitudes chosen ought to be very distant from one another, one near the equator and one near the pole, in order that the curvatures of the meridian may be as different as possible. In places not remote, the proportion of the lengths measured would approach so near the proportion of the observed differences of latitude, that the unavoidable errors of observation would render the result quite uncertain.

Let us next suppose that the geodetical line is perpendicular to the meridian. In this case, $\sin \mu = 1$, $\cos \mu = 0$, $\sin \beta = \sin l$, and $\Delta = \sqrt{\sin^2 l - \sin^2 u}$.

Wherefore, if we put $\sin u = \sin l \cos z$, then

$$\Delta = \sin l \sin z,$$

$$ds = \frac{(1+e^2) \sqrt{1 + e^2 \cos^2 l} \, . \, dz}{(1 + e^2 - e^2 \sin^2 l \cos^2 z)^{\frac{3}{2}}} .$$

In this equation s and z increase together from zero. If it be expanded we shall obtain a formula for computing s as in the foregoing instance. I shall not however take the trouble of further developing the expression, because it is not proper to be employed in the research of the figure of the earth. The reason is, that the small arc z is determined by its cosine; so that a minute error in the latitude u would occasion an excessive variation in z. When a geodetical line is perpendicular to the meridian, the variation of latitude is at first proportional, not to the length measured, but to the square of the length; and therefore it cannot safely be employed in so delicate a research as the deviation of the figure of the earth from a sphere.

In the most general case when the geodetical line is inclined to the meridian in any proposed angle, we must make,

$$\sin u = \sin \beta \cos z.$$

And here it is evident that the determination of the arc z will be liable to the same objection as in the perpendicular to the meridian, unless $\sin u$ is considerably different from $\sin \beta$. It follows therefore that a geodetical line must not make a great angle with the meridian, at least if we employ the difference of latitude in the research. The inclination to the meridian ought not to exceed 45°. On this supposition the arc z will be tolerably well ascertained, and the formula (C) will be sufficient for finding the length of s by means of that arc. The expression would however be a little complicated on account of the number of quantities that enter into it; but as an instance of such an oblique measurement has neither actually occurred, nor can any good reason be given for carrying it into execution, I shall not pursue the subject further.

I have now considered very particularly the problem of the figure of the earth as it depends upon the lines measured on the surface, and the observed differences of latitude. It follows that observations made in the direction of the meridian are the most advantageous for obtaining the values of the quantities sought. When the lengths measured extend only to a few degrees, we may use the differential equation before found, viz.

$$ds = \frac{(1+e^2)\,du}{(1+e^2\cos^2 u)^{\frac{3}{2}}},$$

instead of the integral. In this case, ds or s is the length measured; du, or $u-l$, the difference of latitude in degrees; and if m denote the degrees in the arc equal to the radius (57°·29578), then $\frac{ms}{u-l}$ will be the radius of a circle in which an arc equal to s contains $u-l$ degrees. Hence if P be the polar semi-axis in the same parts with s, and if we take the mean latitude $\frac{l+u}{2}$ for u, we shall have

$$\frac{ms}{u-l} = \frac{P(1+e^2)}{(1+e^2\cos^2\frac{l+u}{2})^{\frac{3}{2}}}.$$

and by expanding the radical and retaining only the first power of e^2, we get,

$$\frac{ms}{u-l} = P\left\{1 + \frac{e^2}{4} - \frac{3e^2}{4}\cos(l+u)\right\}.$$

Two such equations are required for determining P and the ellipticity $\frac{e^2}{2}$.

But in determining the figure of the earth by means of terrestrial observations, instead of the difference of latitude, we may employ the difference of longitude of the two extremities of the line measured, or the change in azimuth at the same stations. And in the case of a perpendicular to the meridian, one or other of the two quantities mentioned must be used, since it has been shown that the difference of latitude is inadequate to the purpose. It therefore becomes necessary to form the expressions of a geodetical line in terms of the difference of longitude, and in terms of the azimuth at its further extremity; but, as this would make too great an addition to what I have already written, I shall reserve it for a future communication.

April 5, 1826. J. IVORY.

XXXIX. *On the Phænomena connected with some Trap Dykes in Yorkshire and Durham.* By the Rev. ADAM SEDGWICK, *M.A. F.R.S. M.G.S. Fellow of Trinity College, and Woodwardian Professor in the University of Cambridge.*

[Concluded from p. 219.]

Extent and Position.

NO other dyke has, I believe, been yet described, which intersects so many secondary formations, and preserves such an extraordinary uniformity of direction and inclination. The whole length, reckoning from the quarry at Gaundlass Mill, is more than fifty miles: and if any one should object to this, as including a considerable space in which the continuity is not apparent, there will still remain from Coatham Stob a distance of about thirty-five miles, through which it is almost certain that the trap ranges without any break or interruption. Perhaps it might with more justice be objected, that the first computation falls below the truth; in consequence of the probable extension of the dyke to the N.W. through the Woodland Fells and Egglestone Burn to the banks of the Tees. Should this supposition be admitted, we shall have an uninterrupted dyke extending from High Teesdale to the confines of the eastern coast, a distance of more than sixty miles.

The angle at which it cuts the strata is of course variable, and in many places cannot possibly be ascertained. At Barwick, near the Tees, its inclination to the horizontal beds of sandstone is more than eighty degrees; and the angle at which it intersects the beds of shale and sandstone in the eastern moors is still greater; occasioned, perhaps, by the south-eastern dip, which generally prevails among the strata in that region *.

Secondary formations, when interrupted in the manner above described, seldom preserve the same level on the opposite sides of their line of separation. Thus at Cockfield Fell, the coal-beds on the north side of the dyke are eighteen feet below the corresponding beds on the south side. In the excavations at Preston and Barwick there is no indication of any great change having been produced in the relative level of the beds of sandstone; nor can any conclusive evidence be obtained on this subject from the obscure sections exhibited by the quarries in the eastern moorlands. Perhaps, as a general rule, the greatest dislocations are produced by those fissures into which trap is not intruded: such at least appears to be the case in the great coal-field of Northumberland and Durham. The injected masses of trap may be supposed to have acted as a kind of support, and to have partially hindered the broken ends of the strata from sliding past each other.

Structure of the Dyke.

Notwithstanding the great length of the Cleveland dyke, and the different nature of the rocks with which it is associated, it undergoes very little modification in its general structure. Its prevailing character is that of a fine granular trap rock of a dark blueish colour. This colour is indeed, with some unimportant exceptions, so constant in all the sound specimens, that the dyke is provincially termed blue-stone by the men who are employed in working the quarries. It breaks into irregular, sharp, angular fragments; and on a recently exposed surface there generally may be seen a number of minute brilliant facets: but the constituent parts are never sufficiently distinguished from each other to give it the appearance of a green-stone. The essential ingredients of the rock are, if I mistake not, pyroxène and felspar, in which respect it agrees with the greater number of trap dykes which have been carefully examined, as well as with a great many varieties of recent lava. The principal modifications, of course, arise from the variable proportions of these essential ingredients. Among the prevailing and nearly compact portions of the dyke, there are some larger crystals of felspar and carbonate of lime; very rarely, however, in such abundance or order of arrangement, as to give any decided appearance of porphyritic structure. Good specimens of amygdaloid are not common; where they do occur, the nodules are chiefly composed of carbonate of lime. In one or two instances we found chalcedony filling the hollows of an imperfect amygdaloid. Iron pyrites may be mentioned among the minerals frequently associated with the dyke. It is found disseminated through the substance of some decomposing varieties in considerable abundance; and small spangles of it may occasionally be seen in the sound specimens, especially among the larger crystals of felspar before mentioned. All the dark sonorous specimens act strongly on the magnet; but some of the light-coloured varieties, which contain a great excess of decomposing felspar, do not sensibly affect it.

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* See the Survey of the Yorkshire coast by Young and Bird.

The dyke is generally separated by a number of natural partings into large blocks, which are amorphous, prismatic, or globular. Near the centre they are sometimes of such entire irregularity as to defy all description. Not unfrequently, however, in the midst of this confusion we may observe traces of a prismatic form; and where this arrangement is most complete the prisms are always transverse to the dyke. Good examples of this form may be seen in the quarry of Preston, and in other localities above described. The sides of the dyke are generally occupied by clusters of minute horizontal prisms, which are often seen in great perfection even where the central mass is amorphous. In the great quarry of Bolam, where the trap has extended laterally over the horizontal beds of sandstone and coal shale, the capping of basaltic rock is divided into rude columns which are perpendicular to the strata on which they rest; and, therefore, nearly at right angles to the prismatic blocks which lie across the leading dyke. This arrangement is exactly similar to that which takes place among some masses of ancient lava near Mount Vesuvius*.

Traces of the globular structure are often visible, especially where the trap passes into an earthy state: for many of the larger blocks, whether prismatic or amorphous, decompose in concentric crusts, which easily fall off and expose the hard spherical nuclei.

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* Altered beds of coal in contact with trap sometimes exhibit a similar arrangement. Thus at Coley Hill (Geological Transactions, vol. iv. p. 23), a small bed of coal abuts against a dyke of basalt, and near this contact the

These balls are particularly abundant in the old quarry of Coatham Stob, and are associated with some blocks of a light gray colour, which have an earthy fracture. Both these varieties are interesting. Some of the balls contain a considerable quantity of olivine, which is, if I mistake not, a very rare mineral in all the other localities. The light-coloured blocks have a superabundance of decomposing felspar, and are partially porphyritic. Carbonate of lime exists in the form of distinct crystals, and is also disseminated through the mass; and in some instances small spherical concretions of compact felspar are found in a congeries of very minute crystals, giving to such specimens the appearance of an amygdaloidal structure. In other cases the concretions effervesce when first plunged into acids, are opaque from the admixture of impurities, and do not possess the characters of a simple mineral.

Effects of Decomposition.

In this dyke, as in almost every similar formation, the effects produced by decomposition are exceedingly varied. The component parts, from the centre to the surface, are in some quarries hard and sonorous. In others, the sides are invested with a ferruginous earthy matter which only penetrates to the depth of a few inches, and gradually passes into a sonorous granular rock. Not unfrequently, a decomposing crust of more considerable thickness covers the surface even of the blocks which are derived from the centre of the dyke. A number of white spots, probably resulting from decomposing felspar, are often disseminated through these earthy masses, and enable us to separate them from other argillaceous materials, with which they are sometimes in contact. It would be a laborious, and not a very profitable task, to attempt a minute account of phænomena like these which vary with every different locality.

Effects produced by Contact of the Dyke.

It now remains to describe some of the effects produced by the intrusion of the dyke. These effects will of course vary with the substances which are acted on. In some of the quarries

the coal is deprived of its bitumen, and arranged in beautiful small *horizontal prisms*. Under the overlying mass in the quarry of Bolam, the carbonaceous shale is rudely prismatic: and in one or two places where this structure is best exhibited, the prisms are nearly *vertical*.

which have been already described, the trap passes through horizontal beds of slate-clay, and the changes produced by its presence are in all these cases strikingly similar. At Nunthorp and Langbargh these beds of slate-clay belong to the great *alum-shale formation* (*lias*), and are easily identified by the belemnites, pectinites and other characteristic fossils which are imbedded in them. On approaching the dyke they become much indurated, and are divided by a great many vertical fissures, which, when combined with the ordinary cleavage, separate the strata into rhomboidal fragments. In all such cases the rifts and fissures are coated over with oxide of iron. In other instances, the true horizontal cleavage entirely disappears; and the indurated masses might then be easily mistaken for beds which had been tilted out of their original position. The alteration produced in the coal-shale at Gaundlass Mill is exactly analogous to what has been described, though not so strikingly exhibited.

In the quarry at Barwick, on the right bank of the Tees, the vein of trap is well denuded, and the south side of the section exposes a great many horizontal beds of sandstone, which are separated into prismatic blocks by a number of natural transverse fissures. Close to the dyke this structure disappears; the sandstone is much more compact, and breaks into amorphous fragments.

It must however be allowed that in some other localities the sandstone did not, under similar circumstances, appear to have undergone any modification.

Perhaps, as a general rule, none of the changes above described are well exhibited, where the portion of the dyke, in contact with the horizontal beds, assumes the appearance of a wacké. Should this observation be sufficiently verified, it would seem to indicate, that the earthy texture of the dyke is, in some cases, rather due to its original mode of aggregation, than to any subsequent decomposition. I may, however, assert unequivocally, that I never saw any beds which are easily susceptible of modification (such as coal or carbonaceous shale) in immediate contact with the trap, without having undergone a remarkable change.

The overlying trap at Bolam bears no resemblance to a substance which has been tranquilly deposited on the inferior strata; for it is separated from them by a broken indented superficies which has exposed many distinct beds to its immediate action. Some of the massy columns rest on a bed of shale partially converted into a substance resembling Lydian stone, which rings under the hammer, or flies in all directions into a number of sharp splinters. Others are supported by a bed of impure coal or carbonaceous shale, in the upper part of which are found shapeless masses in various states of induration, mixed irregularly with angular pieces of trap, and an earthy substance like soot or pounded charcoal. Where the carbonaceous ingredients are most abundant, the parts of the bed in immediate contact assume the appearance of coke derived from the artificial distillation of impure coal, and not unfrequently separate into a number of minute prisms*. An impure carbonaceous powder is sometimes found in the crevices between the basaltic columns, several feet above the beds on which they rest.

In addition to the substances above described, I found beneath the trap some thin white porcellanous fragments, which appeared to be derived from an indurated bed of *fire-clay*—a well-known associate of the great coal formation.

All these phænomena so exactly resemble the effects produced by fire, that I am unable to describe them without using language which may be thought hypothetical by those who deny the igneous origin of trap dykes.

* In Cockfield Fell the coal-works have been conducted on both sides of the dyke, and the extraordinary changes produced by its influence have been recorded by practical men who had no theory to support, and who founded their opinions upon actual observation. The works are not now carried on in the immediate neighbourhood of the dyke; but I procured so many specimens of the substances which had been taken from the altered coal-beds, that I have no doubt of the general accuracy of the accounts which have been published.

Close to the dyke, the *main coal* is converted into a substance resembling *soot*, and at some distance it passes into a more solid substance, which the miners call *cinder*. At a still greater distance it retains a part of its bitumen, and about thirty yards from the trap it does not differ from the ordinary pit-coal of the district. It is stated (Hutchinson's History of Durham) "that immediately above the *cinder* there is a great deal of sulphur in angular forms of a bright yellow colour. The *cinder* burns clear, without smoke, and affords very little sulphurous effluvia."

Igneous Origin of the Dyke.

Were there no other examples of corresponding phænomena, it would perhaps be unsafe to draw any direct conclusions from the facts which have been stated. But in different parts of the British Isles, similar effects appear, in instances almost without number, to have been produced by the operation of similar causes: so that the igneous origin of a large class of trap dykes seems to be established by evidence which is almost irresistible.

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* See the note to p. 251.

It is urged to no purpose, that Lydian stone and glance-coal occur in places which have never been influenced by volcanic action. The assertion may be true, but is of no value in determining the question; unless it can be shown, that substances, similar to those derived from the sides of the dykes, are found in other parts of the same district which are removed from their influence. This, however, is not the case; for the enormous excavations which have been carried on in the great coal-basin of Northumberland and Durham have, with one ambiguous exception*, made us acquainted with no similar substances excepting those which appear to have been produced by similar agents.

General Summary.

It may be proper briefly to enumerate some of the facts which are established by a detailed examination of the great dyke, and which will, perhaps, be considered to place its origin out of all doubt.

1. It is more recent than the formations which it traverses. For it occupies the interval between beds which were evidently once continuous; but have been subsequently broken up and severed by some great convulsion.

2. It was consolidated prior to the last great catastrophe which formed the beds of superficial gravel, and excavated the secondary valleys. In proof of this we need only state, that it partakes of all the inequalities of the districts through which it passes, rising with the hills and falling with the valleys, so that in many of the lower regions it is buried in diluvium. On this subject there is, I believe, no difference of opinion.

3. There is every reason to believe that it has been filled from below. For there exists no trace of any upper bed from which its materials could have been supplied; and in one place, horizontal beds of sandstone rest on the top of a mass of trap which is probably connected with the dyke. We may further

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* See the Geological Transactions, vol. iv. p. 27. The case is obviously ambiguous, because the effect of a large mass of trap on a bed of coal may be propagated to a considerable distance. The very change described by Mr. Winch *may* therefore have been effected by a mass of trap which is not exposed in the workings. We must carefully distinguish between the phænomena here described, and the effects of those dislocations which so commonly intersect the coal strata. In these latter instances the coal beds are often deteriorated on both sides of the line of *fault* by the mere *mechanical* effects of the rupture.

state, that many dykes of similar origin wedge out before they reach the surface*.

4. The dyke has once been in a fluid state. For it is moulded to all the flexures of the chasm which it fills up. The same assertion is also proved by its crystalline texture.

5. The materials of which it is composed are the same with those which abound in a great many varieties of recent lava. On this subject there is perhaps no difference of opinion. For the Wernerians at one time asserted, that recent lava was derived from the igneous fusion of trap rocks of aqueous origin.

6. The effects produced by the dyke are such as might be expected from the intrusion of a great mass of ignited matter. This assertion is fully established by the facts which have been already stated.

If, therefore, similar effects have originated in similar causes, we must conclude, that this dyke, as well as all the other similar masses in the great Durham coal-field, are the undoubted monuments of ancient volcanic action.

Conclusion.

It is a matter of fact, which is independent of all theory, that an enormous mass of strata has been rent asunder; and it is probable that the rent has been prolonged to the extent of fifty or sixty miles. If we exclude volcanic agency, what power in nature is there capable of producing such an effect? By supposing such phænomena the effects of volcanic action, we bring into operation no causes but those which are known to exist, and are adequate to effects even more extensive than those which have been described.

Combining this observation with the facts described with minute detail in the preceding parts of this paper, we obtain a chain of evidence, in favour of the igneous origin of a certain class of trap dykes, not one link of which appears to be defective. It is not to be denied, that the associations of trap rocks may in other cases present great difficulties to the igneous theorist. But these difficulties are not the present subject of consideration. I have confined myself, as far as possible, to a statement of facts, and I have only attempted to record such conclusions as a review of those facts appeared fully to justify.

Trin. Coll. March 12, 1823.

P. S. Before this paper was sent to the press, I received two letters from my friend Mr. Wharton, of Oswald House, near Durham, communicating some very interesting facts connected with the appearance of a basaltic dyke; which ranges from the escarpment of the magnesian limestone (at Quarrington Hill, a few miles to the east of Durham) through the great coal-field, in a direction about W.S.W. It is found along this line at Crowtrees, Tarsdale, Hett, Tudhoe, Whitworth, and Constantine farm. From the last-mentioned place, it passes along the same line of bearing, through the collieries of Bitchburn and Hargill Hill, to a spot near the confluence of Bedburn Beck and the river Wear, where it is well exposed on the surface of the ground; and it is known to pass up the Bedburn Beck valley towards Egglestone Moor. If prolonged a few miles in the same direction, it must meet the line of the Cockfield Fell dyke within a short distance of Egglestone; and may, perhaps, be a prolongation of one of the masses of trap described in a former part of this paper.

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* See Professor Henslow's paper on the Isle of Anglesea; Dr. MacCulloch on the Hebrides, &c. &c.

This dyke is laid down in none of our geological maps. Indeed its existence was probably unknown before Mr. Wharton ascertained its continuity, by examining the thickness, the dip, and the bearing, of several masses of trap, which appeared in separate quarries, but in the same general line of direction. That its further extension towards Egglestone Moor, and its probable connexion with the trap of High Teesdale, should be correctly determined, is certainly an object of considerable interest.

The following facts appear of most importance in illustrating the natural history of this dyke.

1. The trap, in colour, fracture, and external form, is similar to that of Cockfield Fell: It often parts into irregular prismatic blocks with well defined angles, and four or five plane sides covered with an ochreous crust.

2. The width of the dyke appears to increase in its progress westward. Thus, at Crowtrees quarry it is six feet and a half wide,—at Tarsdale quarry nine feet and a half,—at Bitchburn bank fifteen feet,—and still further west it is seventeen feet wide.

3. It dips to the north at an angle which brings it up in a direction which is nearly perpendicular to the coal strata; which, on the north side of the dyke, are found about twenty-four feet above the level of the corresponding beds on the south side.

4. In the collieries situate in its line of direction (viz. Crowtrees, Bitchburn, and Hargill Hill) the seams of coal near the dyke are charred, or converted into a hard mass of cinders; in consequence of which, the works have in some cases been partially abandoned.

5. The dyke appears to decrease in width as it rises towards the surface. Thus, in Crowtrees colliery, the width of the dyke, where it is cut through at the depth of fifteen fathoms, is nearly twice as great as at the surface.

6. It does not appear at Quarrington Hill to cut through a bed of sand and pebbles, which lies between the highest beds of the coal-formation and the magnesian limestone.

The importance of these facts in confirming the theoretical views given in the preceding paper, is too obvious to need any explanation.

Mr. Winch asserts (Geological Transactions, vol. iv. p. 25), " that he has never been able to trace any of these basaltic veins into the magnesian limestone, and is almost certain that, with other members of the coal-formation, they are covered by it." The dyke just described affords some additional evidence in support of this opinion. Moreover, it appears, in its general relations, to agree so exactly with the Cockfield Fell dyke, that I now cannot help suspecting, that this latter also belongs to the class of " basaltic veins " which do not pass up into the magnesian limestone, though I inclined to a different opinion when the preceding paper was written.

Respecting the prolongation of the Cockfield Fell dyke through the region of the magnesian limestone, there are conflicting probabilities which lead to directly opposite conclusions. The near agreement in the direction and dip of the Cockfield Fell and Cleveland dykes, has generally been supposed to afford sufficient evidence for their continuity. If this opinion be adopted, we must, I think, be compelled to admit the existence of a dyke through all the intermediate district*. On the contrary, there is no direct evidence for the existence of any trap associated with the magnesian limestone; and the relations of all the analogous formations in the coal district seem to prove, that the Cockfield Fell dyke cannot pass out of the limits of the coal-formation.

If we adopt this latter opinion, we must admit that the dykes of Cockfield Fell and Cleveland (notwithstanding the agreement in their line of direction) belong to two distinct epochs. After all, the question is only one of local interest; and, as far as regards the leading object of this paper, of no importance whatsoever.

Through the kindness of T. R. Underwood, Esq. of Paris, I have become acquainted with the results of an examination of specimens from several English trap dykes by Professor Cordier. I will subjoin his description of such specimens as were derived from localities alluded to in the preceding paper.

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* See the observations at p. 217 of this paper.

No. 1. From Preston quarry in the Cleveland dyke. *Mimosite*, fine grained, imperfectly porpheroidal from the salient crystals of pyroxène. It is a *basalt* of the ancient mineralogists. The specimen contains a great abundance of dark-greenish gray felspar, mixed with a very small quantity of pyroxène and titaniferous iron. Some points of pyrites are to be seen. The *paste* also envelops laminar crystals of felspar, having a considerable lustre, which give the *paste* a scaly appearance which distinguishes it from *basalt*.

No. 2. From Coaly Hill dyke near Newcastle. *Mimosite*, small grained, passing into *xerasite*. Many of the cavities contain *green-earth*. It is imperfectly porpheroidal. The crystals of felspar very brilliant.

No. 3. From Walbottle Dean dyke. This has a more decided character of a *dolerite*, very fine grained, the felspar whiter than in the others.

As these distinctive terms are not generally adopted by English mineralogists; it may be proper to state that *mimosite* and *dolerite* are granular rocks. *Xerasite* and *basalt* are composed of the same elements, but microscopic, and having the appearance of a *paste*.

XL. *On the Determination of the General Term of a New Class of Infinite Series. By* Charles Babbage, *Esq. M.A. Fellow of the Royal Societies of London and Edinburgh, and of the Cambridge Philosophical Society*[*].

THE subject of investigation on which I have entered in the following paper, had its origin in a circumstance which is, I believe, as yet singular in the history of mathematical science, although there exists considerable probability, that it will not long remain an isolated example of analytical inquiries, suggested and rendered necessary by the progress of machinery adapted to numerical computation. Some time has elapsed since I was examining a small machine I had constructed, by which a table, having its second difference constant, might be computed by mechanical means. In considering the various changes which might be made in the arrangement of its parts, I observed an alteration, by which the calculated series would always have its second difference equal to the unit's figure of the last computed term of the series; other forms of the machine would make the first or the third, or generally any given difference equal to the unit's figure of the term last computed; and a further alteration would make the same difference equal to double, or generally to (*a*) times the digit in the unit's place: or if it were preferred, the digit fixed upon might be that occurring in the ten's place, or generally in the *n*th place. I did not, at that time, possess the means of making these alterations which I had contemplated, but I immediately proceeded to write down one of the series which would have been calculated by the machine thus altered; and commencing with one of the most simple, I formed the series.

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[*] From the Cambridge Philosophical Transactions, vol. ii. Part I.

Series.	Diff.
2	2
4	4
8	8
16	6
22	2
24	4
28	8

If u_z represent any term of this series, then the equation which determines u_z is

$$\Delta u_z = \text{unit's figure of } u_z,$$

an equation of differences of a nature not hitherto considered, nor am I aware that any method has been pointed out for the determining u_z in functions of z from such laws. I shall now lay before the Society, the steps which I took for ascertaining the general terms of such series, and of integrating the equations to which they lead. I shall not, however, commence with the general investigation of the subject, but shall simply point out the paths through which I was led to their solution, conceiving this course to be much more conducive to the progress of analysis, although not so much in unison with the taste which at present prevails in that science.

If we examine the series, and its first differences, it will be perceived, that the terms of the latter recur after intervals of four, and that all the changes in the first differences, are comprised in the numbers 2, 4, 8, 6, which recur continually, and the series may be written thus:

Series.	Diff.
2	2
4	4
8	8
16	6
$22 = 20 + 2$	2
$24 = 20 + 4$	4

28 =

	Series.	Diff.
	28 = 20 + 8	8
	36 = 26 + 16	6
	42 = 40 + 2	2
10	44 = 40 + 4	4
	48 = 40 + 8	8
	56 = 40 + 16	6
	62 = 60 + 2	2
	64 = 60 + 4	4
15	68 = 60 + 8	8
	76 = 60 + 16	6
	82 = 80 + 2	2

If then z be of the form $4v + i$, the value of u_z will be $20v$ + one of four numbers 2, 4, 8, 16, according to the value of i, and if i always represents one of the numbers 1, 2, 3, 4, the value of u_z will be thus expressed,

$$u_z = 20v + 2^i.$$

As a second example, let us consider the series whose first term is 2, its first difference 0, and its second difference always equal the unit's figure of the next term; its equation will be

$$\Delta^2 u_z = \text{unit's figure of } u_z,$$

and the few first terms are

2	28
2	48
4	76
10	110
16	144
	182

This series may be put under the form

	Series.	1 Diff.
0	2	0
	2	2
	4	6
	10	6
	16	12
5	28	20
	48	28
	76	34
	110	34
	144	38
10	182	40 = 40 + 0
	222	42 = 40 + 2
	264	46 = 40 + 6
	310	46 = 40 + 6
	356	52 = 40 + 12

Table of ($\bar{a}$).

if a = 0	($\bar{a}$) = 2
1	2
2	4
3	10
4	16
5	28
6	48
7	76
8	110
9	144

	Series.	1 Diff.
15	408	60 = 40 + 20
	468	68 = 40 + 28
	536	74 = 40 + 34
	610	74 = 40 + 34
	684	78 = 40 + 38
20	762	80 = 80 + 0
	842	82 = 80 + 2
	924	86 = 80 + 6
	1010	86 = 80 + 6
	1096	92 = 80 + 12
25	1188	100 = 80 + 20
	1288	108 = 80 + 28
	1396	114 = 80 + 34
	1510	114 = 80 + 34
	1624	118 = 80 + 38
30	1742	120 = 120 + 0
	1862	122 = 120 + 2

In this series it may be observed, that u_z when z is less than 10, is equal to the sum of the first differences of all the preceding terms; and if z be greater than 10, it will be composed of four terms, viz. first the sum of the ten first terms of the first difference, multiplied by the number of tens contained in z; secondly, of the sum of the series 40 + 80 + 120 + to as many terms as there are tens in z, this must be multiplied by 10, as each term is ten times added; and thirdly, of the number 40 multiplied by the same number of the tens, and also multiplied by the digit in the unit's place of z; and fourthly, of the sum of so many terms of the series as is equal to the unit's figure of z; this being expressed by $(\bar{a})$ signifying the number opposite a in the previous table. These four parts, if $z = 10b + a$, are thus expressed,

1st $180\,b$,

2nd $40\,\frac{b\,.\,\overline{b-1}}{2}\,10$,

3rd $40\,b\,a$,

4th $(\bar{a})$.

These added together produce

$$u_z = 20\,b\,(10\,b + 2\,a - 1) + (\bar{a}).$$

This value of u_z, if diminished by 2, is equal to the sum of $z - 1$ term of the series which constitute the first difference.

This inductive process for discovering the *n*th terms of such series, might be applied to others of the same kind; but it does not admit of an application sufficiently general or direct, to render it desirable that it should be pursued further.

If we consider any series in which the first difference is equal to the digit occurring in the unit's place of the corresponding term, as for example, the series

6	6
12	2
14	4
18	8
26	6
32	2

a slight examination will satisfy us, that the value of the digit occurring in the unit's figure of u_z, depends entirely on the value of u_z, at the commencement of the series, and also that whenever the same digit again occurs, there will, at that point, commence a repetition of the same figures which have preceded; consequently, the first difference at those two points will be equal.

In the first example which I have adduced of a series of this kind, it will be found, that this reappearance of the terminal figure, happens at the 5th, at the 9th, at the 13th terms, &c. or that

$$\Delta u_1 = \Delta u_5 = \Delta u_9 = \Delta u_{13} = \ldots .$$

This gives for the equation of the series,

$$\Delta u_z = \Delta u_{z+4},$$

or by integrating

$$u_z = u_{z+4} + b,$$

but when $z = 1$, $u_1 = u$, therefore $b = 0$, and

$$u_{z+1} - u_z = 0,$$

whose integral is

$$u_z = a(-\sqrt{-1})^z + b(-\sqrt{-1})^{z+1} + c(-\sqrt{-1})^{z+2} + d(-\sqrt{-1})^{z+3} + 5z.$$

The four constants being determined, by comparing this value of u_z with the first four terms of the series, we shall find

$$a = 0, b = -5, c = \tfrac{1}{2} - \sqrt{-1}, \quad d = \tfrac{1}{2} + \sqrt{-1},$$

and the value of u_z becomes

$$u_z = 5(z-1) + (\tfrac{1}{2} - \sqrt{-1})(\sqrt{-1})^z + (\tfrac{1}{2} + \sqrt{-1})(-\sqrt{-1})^z,$$

which expresses any term of the series

2, 4, 8, 16, 22, 24, 28, 36, 42, 44, 48.

It is necessary, for the success of this method, that we should have continued the given series until we arrive at some term whose unit's figure is the same as that of some term which has preceded it: now if we consider that this figure depends solely on that of the one which occupied the same place in the preceding term, it will appear that the same digit must reappear in the course of ten terms at the utmost, since there are only ten digits, and that it may re-occur sooner. The same reasoning is applicable to the case of series whose first difference is equal to any multiple of the digits found in the unit's place of the corresponding term, or to those contained in the equation

$$\Delta u_z = a \times \text{(unit's figure of } u_z\text{)},$$

as also to those in which this is increased by a given quantity, as

$$\Delta u_z = a \text{ (unit's figure of } u_z) + b.$$

If the second difference is equal to some multiple of the figure occurring in the unit's place of the next term, as in the series

$$2, 2, 4, 10, 16,$$

already given, since the unit's figure must always depend on the same figure in the first term of the series, and its first difference

2	0
2	2
4	6
10	6
.	.
.	.

whenever those two figures are the same, a similar period must reappear: now as there are only two figures concerned, they can only admit of 100 permutations, consequently, this is the greatest limit of the periods in such species of series.—In the one in question the period is comprised in ten terms. This reasoning may be extended to other forms of series in which higher differences are given in terms of the digits occurring in the unit's, ten's, or other places of u_z or u_{z+1} or elsewhere, but I am aware that it does not in its present form present that degree of generality which ought to be expected on such a subject: probably the attempt to solve directly that class of equations to which these and similar inquiries lead, may be attended with more valuable results.

As the term "*unit's figure of*" occurs frequently, it will be convenient to designate it by an abbreviation; that which I shall propose is the combination of the two initials, and I shall write the above equation of differences thus

$$\Delta u_z = a \, \mathrm{UF} u_z \, . \, . \, . \, . \, . \, . \, . \, . \, . \, . \, (a).$$

This may be reduced to a more usual form by the following method. If S_x represent the sum of the xth powers of unity, divided by ten; then

$$0S_x + 1S_{x+1} + 2S_{x+2} + 3S_{x+3} + 4S_{x+4} + 5S_{x+5} + 6S_{x+6} + 7S_{x+7} + 8S_{x+8} + 9S_{x+9},$$

will represent the figure which occurs in the unit's place of any number x: substituting u_z instead of x, we have

$$\frac{1}{a}\Delta u_z = 0S_{u_z} + 1S_{u_z+1} + 2S_{u_z+2} + \dots 9S_{u_z+9} \dots\dots(b).$$

an equation in which u_z enters as an exponent.

From the previous knowledge of the form of the general terms of the serie[illegible] are considering, it would appear that the general solution of the equations (a) and (b) is

$$u = 9z + cS_z + c_1 S_{z+z} + c_2 S_{z+2} + \dots\dots c_9 S_{z+9},$$

where the constants must be determined from the conditions. In the further pursuit of any inquiries in this direction, much assistance may be derived by consulting a paper of Mr. Herschel's in the Philosophical Transactions for 1818, "On circulating functions."

Amongst the conditions for determining the general terms of series by some relation amongst particular figures, there occurs a curious class, in which, if we consider only whole numbers, the series becomes impossible after a certain number of terms.

Let the equation determining u_z be

$$\Delta u_z = \tfrac{1}{2}(\mathrm{UF}u_{z-1} + \mathrm{UF}u_{z+1}).$$

Then the following series conform to this law,

Series.	Diff.	Series.	Diff.	Series.	Diff.
1	3	4	6	1	9
4	5	10	4	10	1
9		14	4	11	1
		18		12	3
				15	

[illegible] the law is restricted to whole numbers, none of these series [illegible]mit of any prolongation; nor have I, with that restriction, been able to discover any series of the kind possessing more than five terms.

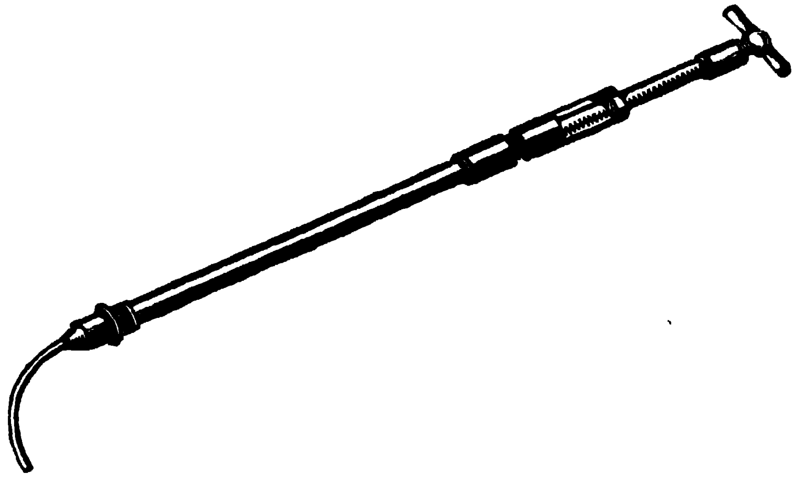
Devonshire Street, Portland Place,
March 29, 1824.

C. Babbage.

XLI. *On the Application of the Sliding Rod Measurement in Hydrometry. By* Robert Hare, *M.D. Professor of Chemistry in the University of Pennsylvania**.

THERE is, in my opinion, no mode of measuring fluids, heretofore contrived, so accurate and convenient, as that which I have employed in my eudiometers. I allude to the contrivance of a rod, or piston, sliding through a collar of leathers into a tube, and expelling from it any contained fluid, in quantities measured by degrees marked upon the rod; and ascertained, with additional accuracy, by means of a vernier.

One of the most advantageous applications of the mechanism alluded to is, in ascertaining specific gravities, in the case either of liquids or solids. To assay liquids which are not corrosive, I have employed two instruments like that represented in the following figure, severally graduated to 100 degrees, and furnished with a vernier, by which those degrees may be divided into tenths, and each scale made equivalent to 1000 parts.



In order to avoid circumlocution, I shall, to the instrument here represented, give the name of *Chyometer;* from the Greek *chuo*, to pour, and *meter*, measure.

Supposing two such instruments to be filled, to the extent of the graduation, one with pure water, the other with any spirituous liquid, lighter than water, whose gravity is to be found; let 1000 parts of the liquid be excluded into one scale of a beam, and then exclude into the other scale as much water as will balance it. Inspecting the graduation of the chyometer, from which the water has been expelled, the numbers observed will be the answer sought. For, supposing 1000 measures of alcohol were placed in one scale, if 800 measures of water counterbalance it, the alcohol must be to the water in gravity as 800 to 1000; since it is self-evident, that when any two masses are made equal in weight, their gravities must be inversely as their bulks.

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* Communicated by the Author.

To ascertain the Specific Gravity of a Solid, by the Chyometer.

For this purpose, the body, whose gravity is in question, should be suspended in the usual way, beneath one of the scales of a balance, and its weight in parts of water, at 60° Fahr. ascertained, by measuring from the chyometer, into the opposite scale, as many parts as will balance the body. Being thus equipoised, and a vessel of pure water, at the same temperature as that introduced by the chyometer, duly placed under it; the number of parts of water, competent exactly to cause it to be merged in this fluid, will be the weight of a quantity of water equivalent in bulk to the body. Of course, dividing by the number thus observed, the weight of the body in parts of water as previously found, the quotient will be the specific gravity.

This process ought to be easily understood, since it differs from the usual process only in using measures of water instead of the brass weights ordinarily employed.

The chyometer enables us to make new weights, out of water, for each process.

To ascertain the Specific Gravity of a Corrosive Fluid, by the Chyometer.

The process described in the preceding page, is only applicable where the fluid is not of a nature to act upon the sliding rod. By employing a body,—a glass bulb for instance,—appended from a balance, as in the usual process, we may use water measured by the chyometer, in lieu of weights.

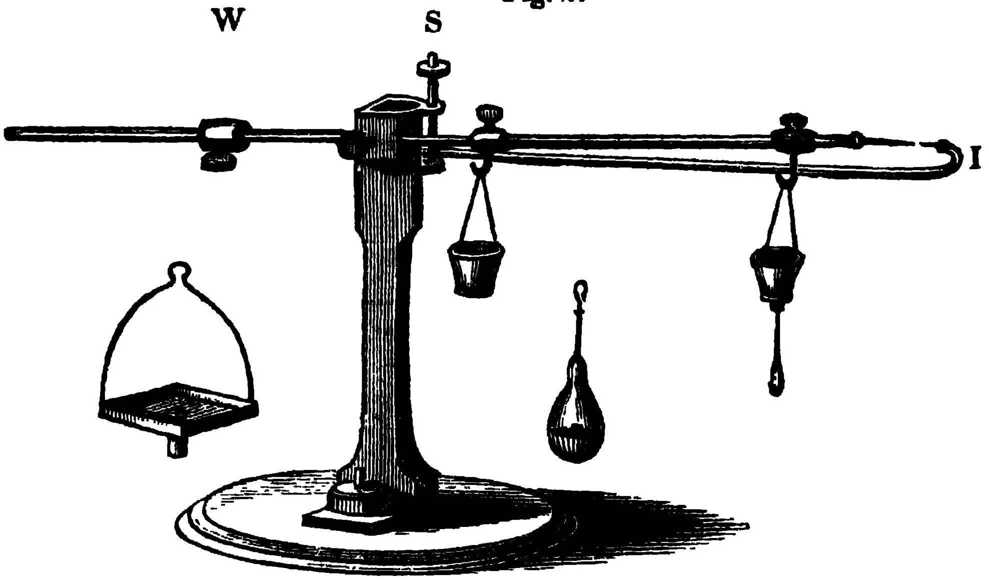
First, having counterbalanced the body exactly, ascertain how many parts of water will cause it to sink in water; next, how many parts will cause it to sink in the liquid whose gravity is to be ascertained. The number last found, being divided by the first, the quotient is the specific gravity.

Supposing that the graduation be made to correspond with the size of the bulb, so that 1000 parts of pure water will just sink the bulb in another portion of the same fluid; the process for any other liquid will be simply to ascertain how many parts of water will sink the bulb in it. The number observed, will be the specific gravity; so that recourse to water, or to calculation, would be unnecessary.

To

To find the Specific Gravity of a Mineral, without Calculation, and without Degrees.

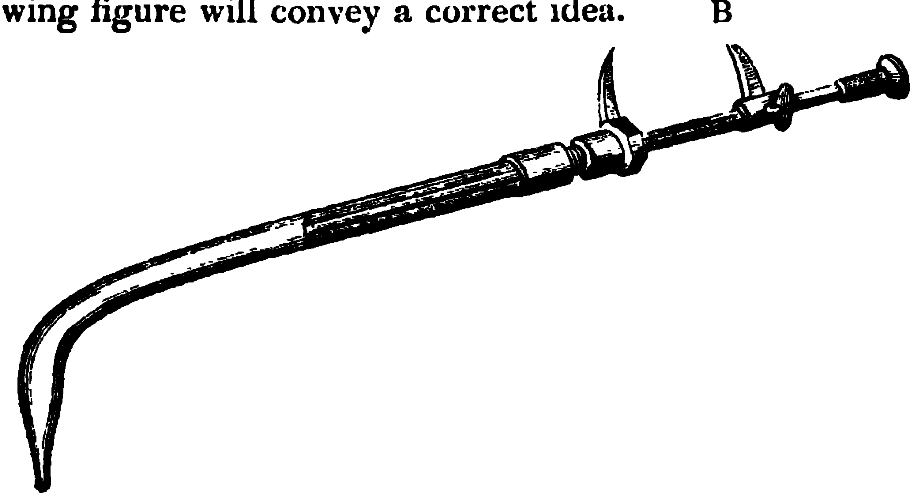
Fig. 2.



The preceding figure represents a balance employed in this process. It is in two respects more convenient than a common balance. The moveable weight on one of the arms, renders it easier to counterpoise bodies of various weights; and the adjustment of the index (I) by the screw (S) to the beam, saves the necessity of adjusting the beam to the index; the accurate accomplishment of which, by varying the weights, is usually a chief part of the trouble of weighing.

One of the buckets, suspended from the beam, is five times as far from the fulcrum as the other.

A chyometer is employed in this process, of which the following figure will convey a correct idea.



The rod of this instrument is not graduated, but is provided with a band (B) which can be slipped along the rod, and fastened at any part of it by means of a screw.

Let a mineral be suspended from the outer bucket, and rendered equiponderant with the counter-weight (W), by moving this further from or nearer to the fulcrum, so that the index point (I) may be exactly opposite the point of the beam. Place under the mineral a vessel of water, and add as much of this fluid to the bucket, by means of the chyometer, as will cause the immersion of the mineral. The band (B) which is made to slip upon the rod, should be so fastened, by means of the screw, as to mark the distance which the rod has entered, in expelling the water, requisite to sink the mineral. Having removed the vessel of water and the mineral, ascertain how many times the same quantity of water, which caused the immersion of the mineral, must be employed to compensate its removal.

Adding to the number thus found, one for the water (previously introduced into the bucket, in order to cause the immersion of the mineral), we have its specific gravity; so far as it may be expressed without fractions. When requisite, these may be discovered by means of the second bucket, which gives fifths for each measure of water; which, if added to the outer bucket, would be equivalent to a whole number. By the eye the distance is easily so divided, as to give half fifths or tenths. Or, the nearest bucket being hung one half nearer the fulcrum, the same measures will become tenths in the latter, which would be units, if added to the outer bucket.

Rationale.

The portion of the rod, marked off by the band, was evidently found competent, by its introduction into the tube of the chyometer, to exclude from the orifice a weight of water, adequate to counteract the resistance encountered by the mineral in sinking in water: consequently, to find the specific gravity of the mineral, we have only to find how often this weight (of water) will go into the weight of the mineral; or, what is the same effect, how often the former must be taken, in order to balance the latter. Indeed it must, otherwise, be sufficiently evident, that the mineral and the water being made equal in weight, their specific gravities must be inversely as their bulks, which are known by the premises.

The inner bucket may be dispensed with, and greater fractional accuracy attained, by means of a sector, graduated into 100 parts. It is for this purpose that the sliding band, and the ferule at the but-end of the tube, are severally furnished with the points. The assistance of a sector is especially applicable, where fluids are in question, since it is necessary to find their differences in thousandths.

To find the Specific Gravity of a Liquid, by the Sectoral Chyometer.

Let a glass bulb (represented in fig. 2, under the buckets) be suspended from the outer bucket, and counterpoised. Let the situation of the beam be marked, by bringing the point of the index opposite to it. Let the tube of the chyometer be full of water, and the rod retracted, until stopped by an enlargement purposely made at its inner termination. Next return it into the tube, until as much water is projected into the bucket, as is just adequate to cause the immersion of the bulb. Let the band be fastened upon the rod, close to where it enters the tube, so as to mark the extent to which it may have entered. The rod must in the next place be drawn out from its tube, to its first position; and the sector so opened, as that the points may extend from 100 degrees on one leg to 100 upon the other. Leaving the sector thus prepared, place under the suspended ball, a vessel containing an adequate quantity of the fluid, whose gravity is required. If the fluid be lighter than water, in order to cause the immersion of the bulb in it, the rod will not have to enter so far, as at first. This distance being marked, by fixing the sliding cylinder, and the rod withdrawn from the tube as far as allowed by the stop, the number on each leg of the sector, with which the points will coincide, gives the gravity of the fluid. Forcing as much water into the bucket as had been sufficient to sink the bulb in water, will not sink it in a heavier liquid; consequently, in the case of such liquids, it will be necessary to fill the chyometer a second time, and force as much more water from it, as may be sufficient to cause the immersion of the bulb. The sliding band being then fixed, and the points separated and applied to the sector as before, the number to which they extend must be added to the weight of water = 100, for the specific gravity of the fluid in question.

Small differences are better found by subtraction; as, for instance, suppose the specific gravity of the fluid were 101; after the small addition of water made to the bucket, beyond the 100 parts required for the immersion of the bulb *in water* (the band being unmoved), the points would extend from 99 on one leg to 99 on the other. The difference between this number and 100, is then to be added to the weight of water; so that the specific gravity is found to be 101.

The angle made by the sectoral lines in using the same bulb and the same rod will always be the same. Hence, a stay may be employed to give the sector the requisite opening.

Indeed, were liquids alone in question, an immoveable sectoral scale would answer. Thus prepared, it were unnecessary to have recourse to water, excepting in the first adjustment of the scale. The number of parts required to merge the bulb in any fluid, will reach (at once or twice) the number or numbers, on the sector, which give the required gravity.

In this process if greater accuracy be desirable, it is only necessary to employ a smaller rod or a larger bulb. Instead of effecting an immersion by one stroke of the rod, it may be done by ten strokes, which will make each division of the sector indicate a thousandth of the bulk of the bulb.

The following process is, however, preferable, as the sector is made to give the answer in thousandths, without the delay of filling and emptying the chyometer more than once.

Let the distance on the rod of the chyometer be ascertained; which, when introduced five times successively, will exclude just water enough to overcome the resistance encountered by a globe, in sinking in that fluid. Let the sector be opened, to the distance so designated: let the globe be partially counterpoised, so as to float in any liquid heavier than 800. The apparatus being thus prepared, if the globe be placed in a liquid, in which it floats, add as much water, from the chyometer to the scale, from which it hangs, as will sink it; and, by means of the points and the sector, ascertain the value of the distance to which the rod has been introduced. Adding the numbers thus found to 800, the sum will be the specific gravity of the liquid.

For this process the sector should be divided into 200 parts; and the proper opening being once duly ascertained, should be preserved by means of an arc like that attached to common beam compasses.

Instead of a globe, a hydrometer surmounted with a cup, may be employed, either with a graduated or a sectoral chyometer.

Before taking leave of the reader, it may be proper to explain the use of the square dish, which may be seen to the left under the beam (fig. 5). The arc of wire is for the purpose of suspending the dish to the hook, in place of the outer bucket. When so suspended, filled with water, and duly balanced, it will be found soon to become sensibly lighter, in consequence of the evaporation of the water. By means of the chyometer, it is easy to ascertain the different quantities evaporated, in similar times, at different periods, and in different places; so that, guarding against the effect of aërial currents, hydrometrical observations may be made with great accuracy.

In lieu of having points attached to the chyometer, as represented in the figure, it may be as convenient to have two small holes, for the insertion of the points of a pair of compasses, either of the common kind, of the construction used by clock-makers, or that which is known under the name of beam compasses.

The compasses may be used to regulate the opening of the sector, or to ascertain by the aid of that instrument, the comparative value of the distances which the rod of the chyometer has to be introduced into its tube.

In order to convey an idea of the nature of the sector to any reader who may be unacquainted with it, I trust it will be sufficient to point out, that its construction is similar to that of the foot-rule used by carpenters. We have only to suppose such a rule, covered with brass, and each leg graduated into 200 equal parts, in order to have an adequate conception of the instrument employed by me.

A more particular explanation of the principle of the sector, may be found in any Encyclopædia, or Dictionary of Mathematics.

XLII. *On the Skeleton of the* Plesiosaurus Dolichodeirus *discovered in the Lias at Lyme, in Dorsetshire, in the Collection of His Grace the Duke of Buckingham.*

THE plate (III.) given in our present Number, represents a nearly perfect skeleton of the *Plesiosaurus Dolichodeirus*, described by the Rev. W. D. Conybeare, F.R.S. &c. in a memoir given at p. 412 of our 65th volume. The drawing from which the original plate in the Geological Transactions was engraved was executed with extreme care by Mr. Webster. The several parts are described in the memoir.

"The bones are entirely imbedded in a matrix of lias shale, which, though intersected in several places by lines of fracture, has evidently, from the mutual adaptation of the parts, formed one entire mass. Above twenty of the cervical vertebræ connected with the head, lie together unbroken.

"We have omitted to state in the memoir, that a second unbroken specimen of the entire vertebral column, from the head to the tail, was found at the same time and place with the one here represented; and has been presented by Professor Buckland to the museum at Oxford."—*Trans. Geol. Soc.* Sec. Ser. vol. i.

[See a delineation of the Skeleton conjecturally restored in Plate III. vol. lxv.]

XLIII. *Note on the Genus* Condylura *of Illiger.* *By* J. D. GODMAN, *M.D.*[*]

AS several very interesting external characters peculiar to the *Condylura cristata* have been entirely overlooked by those who have heretofore written on this subject, the object of this Note is to supply the deficiency as far as possible, especially as these characters may be very serviceable in enabling us to compare the present genus with some others.

The *Condylura cristata* is destitute of an auricle projecting above the level of the skin, but is, nevertheless, provided with an extremely large external ear, as we may properly consider all that part which is entirely exterior to the tympanum and skull. The meatus externus is half an inch long, having a distinctly marked tragus and anti-tragus, and is situated at a short distance from the shoulder, in the broad triangular fold of integument connecting the fore-arm and head, and may be very easily missed by those who merely examine stuffed skins, or specimens preserved in spirits. From the meatus, the course of the cartilaginous tube is obliquely downwards, forwards, and inwards, until it terminates in a delicate bony tube, previous to reaching the tympanum, which is large and composed of a very delicate membrane.

The scales on the anterior and posterior extremities have been mentioned in general terms by several writers, especially by Desmarest, who has given the best description of the animal that has yet appeared. But these scales are so peculiar and uniform in their position, that I cannot understand how a naturalist could pass over the particulars of their arrangement in silence.

On the anterior extremities the superior or ulnar edge of the hand has on its anterior surface, (regarding the position of the animal,) a row of corneous scales, about nine in number, which are broadest midway from the carpus to the first phalanx of the fifth finger. Another row of scales commences on the inferior part of the back of the little finger, becoming broader and of a semilunar figure as they extend towards the metacarpus, between these two a much smaller row is placed. The fourth finger has a single row of small scales on its upper posterior side, and a large one extending along the back of the finger to the metacarpus; the middle finger has a small central row, which is distinguishable; that on the fore finger is still more faint; the thumb has none but very small ones on its central posterior part, but on its inferior posterior part, or radial edge, it has one scale of considerable size on the phalanx, and four or five between this part and the carpus; the two nearest the scale on the phalanx are largest.

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[*] From Journal of Acad. of Nat. Sciences of Philadelphia, vol. v. p. 109.

The surface of the palm of the hand is covered with small circular scales, extending most numerously, and of a darker colour, from opposite the root of the thumb obliquely outward to the basis of the little finger.

On the inferior extremities, the whole of the superior surface of the foot is covered with minute, blackish, circular scales, which increase slightly in size as they approach the toes. On the anterior part of the fourth toe is a large central row of black scales, and on the fifth a rather smaller one; hence these toes have a very considerable resemblance to the toes of a bird. The other toes of the hind foot being applied with their anterior surfaces to the ground, have the scales very minute and almost colourless.

The colour of the scales varies on different parts of the hand. On so much of the back of the hand as is formed by the fourth and little fingers, the scales are very dark blue, approaching a black, in the living animal; thence to the large scales of the thumb the colour changes to a faint purplish blue, which is little more than distinguishable.

Two other excellent characters belonging to the palm of the hand have been neglected: the first is the enlargement of the carpal edge of the palm by an elongation of the integuments; this, in addition to the row of bristles that margins all the rest of the palm, has two distinct bristly hairs at its superior and inferior edge, more than $\frac{1}{8}$th of an inch long. The second character is still more striking; it is a process of the palmar cuticle on the superior edge of the thumb and three succeeding fingers. These processes are serrated and directed obliquely upwards and outwards; the serrations on the thumb being two, and on the three succeeding fingers three in number.

On the soles of the (posterior) feet another character is found, which consists of five circular, distinct spots, so arranged that the two nearest the body are parallel with each other, opposite the commencement of the first toe, counting as in the human subject, from the one nearest the median line of the body; the superior spot is nearly in a line with the fourth toe, and larger and darker coloured than the inferior; the two succeeding spots (nearer the extremity of the toes) are also parallel with each other; the exterior one is largest of all these plantar scales, and placed nearly over the extremity of the metatarsal of the fourth toe; the inferior spot is nearly over the root of the second toe; the fifth or single scale is placed in advance of all the rest, and is situated immediately over the centre and behind the separation of the third and fourth toes.

A very analogous arrangement may be observed in the sole of the feet of the *Sigmodon hispidum* of Ord.

By comparing the Condylura with the Scalops, we are led to several interesting observations. We have seen that the Condylura has a remarkable and large external ear, though it is destitute of a projecting auricle. The Scalops has neither auricle nor meatus externus opening on the side of the head, as the skin of the head extends over the cartilaginous tube, which is small, and a simple funnel. The situation of the ear is to be discovered externally only by a very small spot, not larger than the circumference of an ordinary pin head.

The hand of the Scalops is peculiar for its great breadth and strength: the extraordinary breadth is produced by an additional metacarpal bone, inferior or external to the thumb, articulated with the carpus, and having a tendon for moving it from the common flexor of the fingers*. On the superior or ulnar edge of the hand there is a cartilaginous additament, connected with the little finger by a tendon. The Condylura has the additional metacarpal bone, but rather like a rudiment, and has not the cartilaginous additament at the superior edge of the hand; hence the very great difference in breadth in the hands of the two genera. The Scalops has a slight process or elongation, not at the carpal extremity of the palm, but on the inferior or outer edge of the supplementary bone.

If we compare the Scalops and Condylura with the description of *Talpa europæa*, the resemblance will be found greater between the Condylura and Talpa in regard to the ears and eyes. If we compare the hands and nose, we shall find that the Scalops approximates more closely to the European genus; nevertheless, the affinity of neither is so strong as to endanger their being confounded with Talpa, if we were to judge from external characters alone†.

Of the genus Condylura I believe after a patient examination, and obtaining specimens from various localities, that most probably there is no other species in this country than

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* This structure resembles that of the *Talpa europæa;* but as that species does not exist in this country, I have not been able to obtain a recent specimen for comparison.

† I am happy to state from actual and repeated observation, that it is the Scalops which in this country forms the "*mole-hills,*" similar to those thrown up by the *Talpa europæa*. As far as I can ascertain, no such circumstance has yet been remarked relative to the burrowing of the Condylura. In a forthcoming work on American Natural History, a full account will be given of my observations on the habits of the Scalops and Condylura.

the *cristata**. The only evidence of the existence of a *longicaudata* is that given by Pennant, who describes it without reference to the nasal rays. It is on this indication that Gmelin, Illiger, and Desmarest have allowed of the species, the latter author with very strong doubts, which Ranzani repeats. From Pennant's figure I feel convinced that his *longicaudata* was a stuffed and dried specimen of the *Condylura cristata*, having the nasal radii shrunk and distorted. A specimen in this condition I have now in my possession, and it might readily be taken for the *longicaudata*, figured by Pennant.

. The *Condylura cristata* is subject at certain seasons to a very remarkable enlargement of the tail, varying from the smallest or most ordinary size to the thickness of the little finger. This circumstance was long since made known to many of his friends by Mr. Titian Peale, who found one of the largest size: since then I have found one, and examined several others, and both Messrs. Say and Bonaparte confirm this observation by other examinations: all the specimens yet examined having the tail thus enlarged, were *males;* and it is most probable that the enlargement occurs only during the rutting season. Messrs. Say and Peale both suggested to me a long time since, that the differences heretofore serving for the establishment of the *longicaudata* as a distinct species, were merely sexual. In all other respects the species of Condylura found are invariable in their external characters, if we except a single specimen obtained by my friend, Titian Peale, which may prove to be a new species, should he find other specimens with the same character, for which purpose he defers his observations. It is certainly an extremely desirable circumstance that we should rid the American Fauna of a great number of merely nominal species, which never had existence unless in the imagination of their authors: to this end the labours of American naturalists should be directed, as it is a great advance towards true knowledge to disencumber ourselves of error.

It is well known that the appearance from which Illiger named the genus, was an extravagant exaggeration of Delafaille, who represented it in his plate as having numerous knots or strangulations on the tail. Desmarest's figure is also incorrect in relation to the tail; he having figured it from a dried specimen; in the recent state, the knotted appearance is not distinguishable: he has also drawn it with the palms turned nearly to the earth, instead of placing them with the thumbs to the ground and the palms presenting backwards. In the recent English translation of Baron Cuvier's *Règne Animal*, Desmarest's figure is copied, but is rendered vastly more incorrect and unnatural than it is in the original.

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* A late number of the United States Literary Gazette contained an annunciation of a newly discovered species of this genus, by Dr. Harris, of Milton. From a description given by this gentleman in a letter to a distinguished naturalist of Philadelphia, we are satisfied that the supposed new animal is the well known *Condylura cristata.*

Note.—In my Note on the genus Condylura recently published, it is stated that the Scalops has the integuments continued over the cartilaginous tube leading to the internal ear. I lately had an opportunity of examining several fine specimens, and have found the very small meatus auditorius externus, which will admit a body of the size of a common pin. It is by no means easily discovered, and is situated about three-fourths of an inch behind the eye, nearly over the anterior part of the shoulder joint.

XLIV. *On the Opposition of the Minor Planets.* *By* STEPHEN GROOMBRIDGE, *Esq. F.R.S. &c. &c.*

HAVING computed the apparent places of these planets about the time of their respective oppositions in preceding years, from elements which required correction in the mean epoch of longitude on the orbit; particularly in Pallas, whose mean diurnal tropical motion had been assumed too great a quantity: I have now corrected their elements from the observations made at Greenwich in the last year; and the following Ephemeris will show their apparent places at midnight for 1826.

	Opposition.		Anomaly.	Dist. from ⊖ = 1
Pallas . . .	June 23d at	17^{h} 28'	329° 26'	2·563
Ceres . . .	June 28th	23 59	311 16	1·886
Vesta . . .	August 18th	15 20	256 35	1·291
Juno . . .	November 1st	10 23	166 19	1·023

Pallas will appear very faint, being so distant from the earth; but Vesta and Juno being in the lower part of their orbits, will appear as stars of 6th and 7th magnitude.

Blackheath, April 19, 1826. S. GROOMBRIDGE.

Ephemeris at Midnight.

PALLAS.			CERES.		
1826.	Æ	Dec. N.	1826.	Æ	Dec. S.
	h ′ ″	° ′		h ′ ″	° ′
May 31	18 24 40	23 8½	June 12	18 51 17	26 46
June 1	23 58	13	13	50 57	51
2	23 16	17½	14	49 36	55½
3	22 33	22	15	48 45	27 0
4	21 49	26	16	47 53	5
5	21 5	30	17	47 0	9½
6	20 20	33½	18	46 6	14
7	19 34	36½	19	45 11	18½
8	18 47	39½	20	44 16	23
9	18 0	42	21	43 20	27½
10	17 13	44½	22	42 23	32
11	16 25	47	23	41 26	36
12	15 37	49	24	40 29	40½
13	14 48	50½	25	39 31	44½
14	13 58	51½	26	38 33	49
15	13 8	52½	27	37 35	53
16	12 18	53	28	36 36	57
17	11 28	53½	29	35 37	28 1
18	10 38	53½	30	34 38	5
19	9 48	53½	July 1	33 39	9
20	8 58	53	2	32 40	12½
21	8 7	52	3	31 42	16
22	7 16	51	4	30 44	20
23	6 25	49½	5	29 46	23½
24	5 34	48	6	28 48	27
25	4 43	46	7	27 50	30
26	3 53	43½	8	26 53	33½
27	3 3	41	9	25 57	36½
28	2 13	38½	10	25 1	40
29	1 23	35½	11	24 6	43
30	0 34	32	12	23 11	46
July 1	17 59 45	28	13	22 17	49
2	58 57	24	14	21 25	51½
3	58 9	20	15	20 33	54
4	57 21	15½	16	19 42	56½
5	56 34	10½	17	18 52	59
6	55 48	5½	18	18 3	29 1½
7	55 2	23 0½	19	17 14	4
8	54 17	22 55	20	16 26	6½
9	53 33	49	21	15 40	8½
10	52 49	43	22	14 55	10½
11	52 6	36½	23	14 11	12½
12	51 24	30	24	13 29	14½
13	50 42	23	25	12 48	16½

Ephemeris at Midnight.

VESTA.						JUNO.					
1826.	Æ			Dec. S.		1826.	Æ			Dec. N.	
	h	′	″	°	′		h	′	″	°	′
July 24	22	23	14	17	44	Oct. 12	3	1	14	0	24
25		22	41		52	13		0	52		10½
26		22	7	18	0					South.	
27		21	32		8	14		0	29	0	2½
28		20	57		16	15		0	6		16
29		20	20		24	16	2	59	41		29
30		19	41		32½	17		59	16		42½
31		19	2		41	18		58	51		55½
Aug. 1		18	21		50	19		58	24	1	8½
2		17	38		59	20		57	55		21½
3		16	54	19	8	21		57	24		34½
4		16	9		17	22		56	51		47
5		15	22		25	23		56	17	2	0
6		14	33		33	24		55	42		12½
7		13	44		41	25		55	5		25
8		12	53		49	26		54	28		37
9		12	2		57½	27		53	50		49
10		11	11	20	6	28		53	11	3	1
11		10	19		14	29		52	32		12½
12		9	26		22½	30		51	53		23½
13		8	33		30½	31		51	12		34½
14		7	39		38½	Nov. 1		50	31		45
15		6	44		46½	2		49	50		55½
16		5	49		54½	3		49	9	4	5½
17		4	51	21	2½	4		48	27		15½
18		3	58		10	5		47	45		25
19		3	2		17½	6		47	3		34½
20		2	6		25	7		46	21		43½
21		1	10		32	8		45	39		52
22		0	14		39	9		44	57	5	0½
23	21	59	18		46	10		44	16		8½
24		58	23		53	11		43	35		16
25		57	28		59½	12		42	55		23
26		56	34	22	6	13		42	15		29½
27		55	40		12	14		41	36		36
28		54	46		18	15		40	58		42
29		53	53		23½	16		40	20		47½
30		53	0		29	17		39	41		52½
31		52	8		34½	18		39	10		57½
Sept. 1		51	16		39½	19		38	36	6	2
2		50	26		44½	20		38	3		6
3		49	37		49	21		37	31		9½
4		48	49		53½	22		37	0		12½
5		48	3		58	23		36	30		15
6		47	19	23	2	24		36	0		17½
7		46	37		6	25		35	31		20

XLV. *Report made to the Academy of Sciences, 22d of August 1825, on the Voyage of Discovery, performed in the Years 1822, 1823, 1824, and 1825, under the command of M.* DUPERREY, *Lieutenant of the Navy.*

(*Commissioners: MM.* DE HUMBOLDT, CUVIER, DESFONTAINES, CORDIER, LATREILLE, DE ROSSEL; *and* ARAGO, *Reporter.*)

[Continued from p. 210.]

Magnetism.

THE phænomena of terrestrial magnetism, in spite of more than a century of researches, are still enveloped in great obscurity. M. Duperrey was occupied upon them, during the whole of his voyage, with the most persevering attention, both when at sea and when in various ports. His journals contain a multitude of observations of declination, inclination, intensity, and diurnal variations of the declination, made according to the best methods. The Commission is of opinion that by here presenting a rapid sketch of the advancement which science may expect from this great work, it will fulfill the intentions of the Academy.

There exists, as is well known, on the globe, a curve along which the magnetic needle maintains a horizontal position. This curve, which has received the name of the *magnetic equator*, has been lately the object of the investigations of MM. Hansteen and Morlet. Although these two philosophers have used the same data, yet on some points they have arrived at results slightly different. In the chart of the learned Norwegian, as well as in that of our countryman, the magnetic equator is, entirely, to the south of the terrestrial equator, between Africa and America. The greatest distance of these two curves in latitude, corresponds to about 25° of west longitude: it is of 13° or 14°.

In the first chart we find a node (*nœud*) in Africa, at 22° of east longitude; the second places it 4° more to the west. According to Messrs. Hansteen and Morlet, if we proceed from this node advancing to the side of the Indian sea, the line of no dip swerves rapidly towards the north of the terrestrial equator, quits Africa a little to the south of Cape Guardafui, and comes in the Arabian sea to its absolute maximum of northern excursion (about 12°), at 62° east longitude. Between this meridian and the 174th degree of longitude, the line of no dip constantly keeps in the northern hemisphere. It cuts the Indian peninsula a little to the north of Cape Comorin; crosses the Gulf of Bengal slightly approaching the terrestrial equator from which it is only 8° apart at the entrance of the Gulf of Siam; then remounts a trifle to the north; nearly touches the northern point of Borneo; crosses the isle Paragua, the strait which separates the southernmost of the Philippines from the island of Mindanao, and under the meridian of Walgiou is again found at 9° of north latitude. From thence, after having passed through the archipelago of the Carolines, the magnetic equator descends rapidly towards the terrestrial equator, and cuts it, according to Morlet, at 174°; and according to Hansteen, at 187° of east longitude. There is much less uncertainty respecting the position of a second node also situated in the Pacific Ocean: its west longitude should be about 120°: but whilst the researches of M. Morlet have led him to admit that the magnetic equator, after having merely *touched* the terrestrial equator, immediately inclines towards the south, M. Hansteen supposes that this curve passes into the northern hemisphere for a space of about 15° of longitude, and then returns again to cut the equinoctial line at 23° distance from the western coast of America. In fine, not to exaggerate this discordance, we ought to say that in its northern excursion, Hansteen's curve without dip does not depart from the terrestrial equator more than one degree and a half, and that, definitively, this line, and that of M. Morlet, are nowhere at two degrees distance one from the other in the direction of the parallels of latitude.

These different results belong to the magnetic equator of the year 1780. Have there happened, since then, any remarkable changes, either in the form of this curve, or in the position of its nodes? We do not doubt that the labours of M. Duperrey, united to the excellent observations of M. Freycinet, may fully clear up this question; your commissioners must confine themselves here to laying before you what they have been able to deduce from a first view.

The Coquille has crossed the magnetic equator six times. Two of the points whose position she thus directly determined are situated in the Atlantic Ocean at 27° 19′ 22″ and 14° 20′ 15″ west longitude, and 12° 27′ 11″ and 9° 45′ 0″ of south latitude. In M. Morlet's map the latitudes of the points of the line of no dip answering to 27°¼ and 14°⅓ west longitude, are respectively 14° 10′ and 11° 36′. The line without inclination seems then, at the first point, to have come nearer to the terrestrial equator by 1° 43′, and at the meridian of the second, by 1° 51′. M. Hansteen's chart gives very nearly the same differences.

In the South Sea, near the coast of America, M. Duperrey found, first in going from Callao to Payta, and afterwards during his navigation between Payta and the Society Islands, two points of the magnetic equator, of which the co-ordinates are:

Long. 83° 38′ W.	Lat. 7° 45′ S.
Long. 85° 46′ W.	Lat. 6° 18′ S.

In the charts of MM. Hansteen and Morlet, the latitudes are about one degree *less*. Here the difference is in a contrary direction to that which we found in the Atlantic Ocean: towards the coasts of Peru, the magnetic equator seems then to have *become more distant* from the terrestrial equator.

Let us, lastly, proceed to the two points determined directly during the circumnavigation of the Coquille, in the northern part of the line of no dip. M. Duperrey has found for their co-ordinates:

Long. 170° 37′ 24″ E.	Lat. 0° 53′ N.
Long. 145° 2′ 38″ E.	Lat. 7° 0′ N.

These latitudes are *less* on the charts which represent the equator of 1780. In the part of the equinoctial ocean corresponding to the Carolines and to the Mulgrave Islands, the line of no dip seems now, notwithstanding, *to remove* from the terrestrial equator.

Variations apparently so contradictory, will notwithstanding admit of a very simple explanation, even without its being necessary to admit a change of form in the magnetic equator, provided we suppose that this curve is endowed with a translatory movement, which, from year to year, transports it progressively and in mass from the east to the west. From 1780 to the present period, this retrogradation of the nodes, in order that the numerical value of the change observed in the latitudes may be deduced from it, should hardly be below 10°. If the rapidity of this change of position be looked upon as an objection, we would remark that the direct observations of the position of the nodes lead very nearly to the same results. M. Duperrey has found, in fact, a node of the curve at about 172° east longitude: on M. Hansteen's map this node is placed at the 184th degree. In the South Sea, the tangent node of M. Morlet, and the two nodes of M. Hansteen are found between the 108th and the 126th degree of west longitude. Very exact observations made on board the Uranie, in 1819, and which M. Freycinet has had the goodness to communicate to us, carry this node as far as the 132d degree of longitude. Indeed we find, in a work by Captain Sabine, published only a few weeks since by order of the British Board of Longitude, an observation which shows in a manner no less evident that the point of intersection of the two equators, which was situated in Africa, in the interior of the continent, and pretty far from the coast in 1780, has advanced from the east to the west as far as the Atlantic Ocean. The observation of which we have been speaking was made at the Portuguese island of St. Thomas. Captain Sabine found indeed, in 1822, for the value of the dip, 0° 4′ S. The magnetic equator then actually passes by this island, the latitude of which is 24′ N., or some minutes only more to the west. Its point of intersection with the terrestrial equator is about 5° of east longitude, whilst, according to the observations of 1780, MM. Morlet and Hansteen have placed it at least 13° more to the east.

According to these several approximations, the existence of a translatory movement in the magnetic equator is very probable. M. Morlet had already pointed it out, but with the proper doubt which measures of the dip obtained without change of the poles of the needle justly excited in his mind. In this respect we can now obtain complete certainty in investigating under the same point of view the whole of the observations on the dip made in the open sea in the equinoctial regions. The journals kept on board the Uranie and the Coquille include all the elements of these researches, in our opinion one of the most important that can now be undertaken on the phænomena of terrestrial magnetism. It would appear, in short, that it is the form and position of the line of no dip, which determines from one pole to the other, in what direction, in every place, the annual variations of the magnetic needle shall manifest themselves. This conjecture, inasmuch as there is question of change of inclination, is to be found in the interesting memoir of M. Morlet, which the Academy some years ago honoured with its approbation. If the appellation of magnetic latitude of a point be given to the angular distance from this point to the line without dip, measured on the magnetic meridian considered as a great circle, we shall find in general, according to M. Morlet, that the inclination of the needle *diminishes*, where the translatory movement of the equator tends to *diminish* the magnetic latitude; and that it *increases*, on the contrary, every where where the magnetic latitude *becomes greater*. Some places, such as New Holland, Teneriffe, &c. seem notwithstanding to form an exception to it. The observations collected in the voyages of the Uranie and Coquille have enabled us to submit this rule to a greater number of verifications, and to learn that it agrees with experience in a very remarkable manner, even in the stations which M. Morlet had excepted. We see in this manner that if the south inclination *increases* rapidly at St. Helena, whilst the north inclination *diminishes* rapidly at Ascension, it is because in its translatory movement, the magnetic equator, which is considerably removed from the first of these islands, approaches, on the contrary, the second, which it will even reach in a few years. The magnetic meridian of the Cape, produced towards the north, passes at a little distance from one of the nodes towards the west: hence the inclination must rapidly *increase* there; and this is what the observations of Cook, of Bayly, of King, of Vancouver, and of Freycinet, also show. At Otaheite, Bayly, Wales, and Cook found, in 1773, 1774, and 1777, a dip of the needle of about 30°; M. Duperrey deduces from his observations 30° 36′; the annual change then is nearly insensible: but the magnetic meridian of Otaheite also meets the line without dip very near to its *maximum* of latitude; that is to say, in a point where this curve is nearly parallel to the terrestrial meridian. The rapid change of dip at Conception in Chili, deduced from the comparison of the observations of Malaspina and of M. Duperrey; the inconsiderableness, on the contrary, of this motion at the Sandwich Islands, which seems to us to result from the observations of Bayly, Cook, Vancouver, and M. Freycinet, present a no less striking confirmation of the rule.

If an exact investigation of the observations on the horizontal needle showed, what at first sight appears to be the case, that in each place the changes of variation may also be connected with the position of the magnetic equator, the study of the motion of this curve would acquire a new importance. It is an inquiry of which MM. Freycinet and Duperrey possess all the elements, and which appears to us worthy of occupying their attention. We shall content ourselves here with remarking, that it results from the observations of these two officers, compared with those of Cook and Vancouver, that the declination, whether at Otaheite, to the south of the two equators, or at the Sandwich Islands, in a northern latitude, is still as little variable as the dip.

The maritime expedition of the Uranie is the first during which the diurnal oscillations of the horizontal magnetic needle were studied. The valuable observations published by M. Freycinet have established in an incontestible manner, that between the tropics the extent of this oscillation is sensibly less than in our climates. It would also appear that we may infer that in the southern hemisphere, *whatever be the direction of the dip*, the northern extremity of the needle moves towards the east at the same hour when we see it in Europe vary towards the west. This fact, in its turn, led to the consequence, that between Europe and the regions where M. Freycinet's observations were made, points must be found in which the variation would be absolutely nothing. There remained only to determine whether these points belonged to the magnetic equator or the terrestrial equator. The second supposition could hardly be reconciled with the existence of a diurnal variation of from three to four minutes at Rawack: for this port, situated in the country of the Papous, is only in 0° 1′½ south latitude. Nevertheless it seemed desireable, in order to dissipate all uncertainty, that the phænomenon should be observed between the two equators. Such was the principal object of the stay of M. Duperrey at Payta. In this city, situated to the north of the magnetic equator and to the south of the terrestrial equator, the northern extremity of the needle observed with a microscope moved, as in Europe, from east to west, from eight o'clock in the morning till noon. This angular deviation is very small; but its direction, respecting which the observations leave no uncertainty, would seem to authorise the conclusion, that all along the magnetic equator the horizontal needle is not subject to diurnal variations. In other stations situated like Payta,—at the Isle of Ascension, for example,—we have nevertheless been able to see that this inference would have been premature. The phænomenon is more complex than would be imagined. Perhaps the changes in the declination of the sun, which in Europe occasion such great variations in the extent of the diurnal oscillations, produce, according to the seasons under the tropics, motions of the needle in an inverse direction. Further observations made in months and places suitably chosen will remove these doubts. It appears to us also, that it would be very useful for the Academy, from this time, to recommend this inquiry in a particular manner to the attention of navigators, especially if, as is announced, a new expedition for discovery is soon to sail from our ports.

To terminate this article, the length of which we hope will be excused, we have yet to add that M. Duperrey has given all his attention to the experiments from which may be deduced the comparative intensities of terrestrial magnetism in various places, and that he is also engaged in making observations proper for giving the corrections of which the magnetic elements obtained at sea are susceptible. It has appeared to us that in general these corrections will be very inconsiderable.

Meteorology.

Meteorology will have been enriched by the expedition of the Coquille, from a journal in which, for thirty-one months in succession and without there being one exception, were noted six times a day the state of the atmosphere, its temperature, its pressure, and the temperature of the sea. While lying-to for example, at Payta; at Waigiou, under the terrestrial equator;

tor; at the Isle of France, at St. Helena, at Ascension, between the tropics; our navigators had the incredible patience to observe the thermometer and the barometer at every quarter of an hour, day and night, for whole weeks. So much pains, will not be lost; observations so minutely exact, so detailed will furnish valuable data on the law which connects corresponding atmospheric temperatures with the different hours of the day; on the value of the diurnal and nocturnal barometric period; on the hours of the *maxima* and the *minima*, &c. Thanks to the extreme complaisance of M. Delcros, (a very distinguished geographical engineer,) in going at the request of one of us, to Toulon,—in order to compare the instruments of the Coquille with a barometer which belongs to him, and which has agreed for several years with that of the Observatory,—we shall be able to decide that which indeed is scarcely any longer a question, since the observations of MM. Boussingault and Riviero have been received in Europe, whether the mean pressure of the atmosphere be the same in all climates.

Since the celebrated voyages of Cook, no one any longer doubts that the southern hemisphere is in mass decidedly colder than the northern;—but at what distance from the equinoctial regions does the difference begin to be felt? According to what law does it become greater in proportion as the latitude augments? When these questions shall have been completely resolved, the various causes to which this great phænomenon has been attributed may be submitted to an exact investigation. Already the stay of M. Duperrey at the Malouines, will show that by 51°½ of latitude, the difference of climate is very great. We see, in effect, that at the anchorage of the *Baie Française*, from the 19th to the 30th of November 1822, the mean temperatures of the atmosphere and of the sea were respectively:

$$+ 8^{\circ}\cdot 0 \text{ and } + 8^{\circ}\cdot 2 \text{ Cent.}$$

The month following, from the 1st to the 18th, we found:

$$+ 10^{\circ}\cdot 0 \text{ and } + 9^{\circ}\cdot 4.$$

We can then adopt + 9°· 0 Cent. for the mean temperature of the Malouines, in the thirty days which precede the summer solstice of these regions. London is precisely under the latitude of *Baie Française.* Then the mean temperature of the twelve last days of May, and of the eighteen first days of June, according to the tables published by the Royal Society, is about 15° Cent.: that is, 6° more than at the Malouines.

The inquiry respecting the direction and swiftness of currents merits in the highest degree the attention of navigators.

tors. Meteorological observations are not less adapted to accelerate the progress of this important branch of the nautical art, than the method generally employed by mariners, and which consists in comparing latitudes and longitudes astronomically determined, with the corresponding latitudes and longitudes deduced from the observation of the compass and the log.

The waters of a certain region, when they are transported by a current into a region more or less approaching to the equator, lose in the passage only a part of their former temperature. The ocean is thus furrowed by a great number of streams of warm and cold water, whose existence the thermometer manifests, and points out in a certain degree their direction. Every one knows the researches of Franklin, of Blagden, of Williams, and of Humboldt, on the equinoctial current, which, after being turned back in the Gulf of Mexico, after having issued out through the strait of Bahama, moves from the south to the north, at a certain distance from the eastern coast of America, and proceeds, under the name of the Gulf-Stream, to temper the climate of Ireland, of the Shetland Isles, and of Norway. At the other extremity of this vast continent, along the coasts of Chili and of Peru, a rapid current from south to north carries on the other hand as far as Callao the cold waters of Cape Horn and of the Straits of Magellan. The anomalous temperature of the ocean, in the port of Lima, was remarked as far back as the sixteenth century. Acosta, indeed, says (lib. ii. cap. 2. pag. 70), that liquors may be cooled at Calloa by plunging them in the sea water; but it was M. de Humboldt who first proved, by exact experiments, that this accidental temperature is the effect, in a great degree at least, of a southern current, whose limit is Cape Blanc: more to the north, in the Gulf of Guayaquil, he found no traces of it. The numerous observations collected in the Coquille, either during its navigation along the coasts of Chili and Peru, or during its stay at Conception, at Lima, and at Payta, will furnish important data relative to this curious phænomenon. At Payta, for example, the temperature of the air was in general 5, 6, and even sometimes 7° Cent. above that of the sea. The mean difference of these temperatures, determined by thirteen days observations in the month of March, rises to 5°: during the stay at Callao, a difference was also found in the same direction; but it is less than at Payta, which perhaps would not have been expected. The journals kept in all the other ports, that of Conception in Chili excepted, do not present any thing similar: the water and the atmosphere on an average of ten days observations give very nearly the same degree.

The consideration of the *absolute* temperatures would furnish a proof not less certain of the existence of this current of cold water. At the port of Callao, from the 26th of February to the 4th of March, the mean temperatures of the air and of the sea were respectively 21°·3 and 19°·1 Cent. At sea, at 800 leagues from the coasts, under the same latitude, as also under a higher latitude, they found, from the 7th to the 10th of April, 25°·9 and 25°·6. At Payta, from the 10th to the 22d of March, the mean temperatures of the air and water which we deduce from the journals of the Coquille are 25°·1 and 20°·0. Here the current no longer exercises, as it appears, a very great influence on the temperature of the atmosphere near the coast; but it is still 6 or 7 degrees colder than the ocean at a similar latitude in all other parts of the sea.

We applied ourselves to this investigation of some of the meteorological observations made by M. Duperrey, in order to show how desirable it would be that they should be printed entire; the physical sciences and even the nautical art would derive great advantage from this. May it also be permitted us, in closing this article, to express the regret which we have felt, in not finding in such rich and valuable journals, some observations of the temperature of the sea at great depths. This inquiry, so directly connected with that of the existence of submarine currents, would nevertheless not have retarded the sailing of the Coquille a quarter of an hour, since in general it would have sufficed to have attached a thermometer to the deep-sea-lead every time it was thrown into the sea. If experiments so interesting were completely neglected by M. Duperrey and his fellow-labourers, it is almost needless to say that it was only because the means of making them with exactitude were wanting. There was not indeed on board the corvette one of those ingenious thermometers which mark by indexes the *maxima* and *minima* of temperature to which they have been exposed.

An expedition for discovery seldom leaves our ports without the Academy being consulted by the public authorities, even without their requiring it, to prepare the instructions for the commander. We think that it would contribute in a manner not less efficacious to the progress of the sciences, if it caused to be prepared before-hand, by the most skilful artists, some of the philosophical instruments which the navigators might want. If the Academy, as we hope, shall deign to give effect to the proposition which we have had the honour to make, it will for the future not have to remark any omission in the labours which may be laid before it: and this arrangement will contribute to diffuse the spirit of research and the taste for accuracy, amongst that rising generation, full of talent and of zeal, with which our ports abound.

[To be continued.]

XLVI. *Notices respecting New Books.*

Remarks on the Cultivation of the Silk Worm; with additional Observations, made in Italy, during the summer of 1825. By John Murray, F.S.A., F.L.S. Glasgow, 1825 pp. 29.

THE substance of this useful tract formed an article in the Edinburgh Journal of Science, No. III.; and the author has now made various additions to it, embracing the most material parts of Count Dandolo's work on his improved culture of the silk worm. In an appendix, an account is given of the chemical nature of silk, &c., and some particulars of the history of its use in the manufacture of clothing, together with an outline of its preparation for that purpose, as now practised in Italy.

Descriptive Account of a Shower Bath, constructed on a principle not hitherto applied to that machine; to which is added, that of an apparatus for restoring suspended animation; and an invention for forming a line of communication in shipwreck; and a fire-escape, in cases of fire. By the same Author. Glasgow, 1826.

In Mr. Murray's shower bath, the column of water in the vase above is supported by the resisting atmosphere; and the superiority of his improvement consists, in the numerous repetitions which may be made from the same supply of water:—The intervals may be shortened or prolonged at pleasure, while the duration of each is under the complete control of the patient, and the water may be suffered to fall in a continued shower of any required division of the streams, attenuating even to a gentle dew.

Just Published.

The Zoological Journal, No. viii. completing the second volume: conducted by Thomas Bell, Esq. F.L.S., J. G. Children, Esq. F.R. & L.S., J. D. C. Sowerby, Esq. F.L.S., and G. B. Sowerby, Esq. F.L.S. Also No. II. of Supplementary Plates to the Zoological Journal.

Treatise on Clock and Watch Making, theoretical and practical. By Thomas Reid, Edinburgh, Hon. Mem. of the Worshipful Company of Clockmakers, London.

XLVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 6.—A PAPER was read On observations made with an invariable pendulum at Greenwich, and at Port Bowen; by Lieut H. Forster, R.N., F.R.S.

April 13.—The following papers were read: On the diurnal variation of the needle at Port Bowen; by Capt. W. E. Parry, R.N., F.R.S., and Lieut. H. Forster, R.N., F.R.S.

On the dip of the needle at different latitudes between Woolwich and Port Bowen; by Lieut. Forster.

On the magnetism imparted to iron by rotation; by the same: with remarks by S. H. Christie, Esq. M.A., F.R.S.

April 20.—A paper was read On a formula expressing the decrement of human life; by Thomas Young, M.D. For. Sec. R.S.

LINNÆAN SOCIETY.

April 4.—The following papers were read:—On dichotomous and quinary arrangements in Natural History; by Hen. Thos. Colebrooke, Esq. F.R.S. F.L.S. &c.

The learned author states that what has been called the dichotomous arrangement of nature can only be represented on a superficies: whereas the affinities of natural objects ramify in *every* direction, and cannot therefore be correctly represented on a *plane* surface. He then shows that that distribution which, taking one central or interior group, makes only a few equidistant exterior ones, is *necessarily* quinary. The centres of the exterior groups will represent the solid angles of a tetrahedron within a sphere of which the centre is the middle point in the interior group.—He finally observes, that although the tendency to a quinary arrangement in natural history has hitherto been chiefly developed in zoology, yet the same principle may be recognised in botany.

Also a communication, by the same author, On *Boswellia*, and certain Indian *Terebinthaceæ*. Mr. Colebrooke is of opinion that the three genera *Amyris*, *Icica*, and *Bursera* require to be thrown together and recast: the whole group comprising nearly 40 species, several of which are unpublished. Among those described are *Boswellia serrata*, *Bursera serrata*, *Chalcas nitida*, *Amyris heptaphylla*, *A. punctata*, *Bergera integerrima*, and *B. Kœnigii*.

April 18.—A large collection of the plants of Nepal was presented from the East India Company. The papers read were a continuation of Mr. Colebrooke's on *Boswellia*, and certain Indian *Terebinthaceæ*;—and observations on a species of *Simia*

Simia Linn., now alive in the collection of Exeter 'Change, allied to, if not identical with, the *Simia Lagothrix* of Baron Humboldt; by Edward Griffiths, Esq. F.L.S.

GEOLOGICAL SOCIETY.

March 17.—A paper was read, entitled, "On the strata of the Plastic Clay formation exhibited in the cliffs between Christchurch Head, Hampshire, and Studland Bay, Dorsetshire; by Charles Lyell, Esq. F.G.S. &c.

The strata of sand and clay which form the subject of this communication are referable exclusively to the plastic clay formation. They occupy an interval in the coast of about 16 miles in extent, between the London clay of Highcliff on the east of Muddiford, and the chalk of the Isle of Purbeck. A coloured section of the strata exhibited in these cliffs accompanies the paper. The author first describes in detail the cliffs of Christchurch, or Hengistbury Head, which consist of sand and loam, often much charged with bituminous matter, and containing large concretions of ferruginous sandstone and clay ironstone, disposed in fine parallel layers, in which, as well as in the sand and loam, occur black-flint pebbles, lignite, and flattened impressions of fossil trees. Below these strata are dark bituminous clays, alternating with red and brown sands, and with occasional layers of black-flint pebbles. After the outcrop of the above strata, the cliffs are low, and about three miles from Muddiford are composed solely of diluvium. When they rise again in height, their direction corresponds with the line of bearing of the strata, so that the same beds are continuously exposed for eight miles, as far as the mouth of Poole Harbour.

These beds consist of fine white sand, pinkish sand, and thinly laminated argillaceous marls, containing occasionally much vegetable matter; and the whole series exceeding 150 feet in thickness. The section is interrupted for a space of 2½ miles by the mouth of Poole Harbour and the bars of sand on each side of it. But in the cliffs near Studland the strata are again seen, consisting principally of yellow and purplish sand, white sand alternating with thinly laminated white clay, and sand with ferruginous concretions passing into sandstone, and pipe-clay.

The junction of the chalk with the superior strata is very indistinctly exposed, but a thin bed of striated soft chalk-marl rests immediately upon the chalk, as is the case in Alum Bay. The author concludes with observations on the diluvium of this district, composed chiefly of chalk-flints; and he infers from its local characters, both here and in the rest for Hampshire, as well as in the district between the North and South Downs,

that

that it owes its origin, in this part of England, to causes much more local in their operation than those generally assigned. He examines how far the phænomena attending its distribution are consistent with the supposition, that the diluvium was formed in consequence of the protrusion of the inferior through the superior strata, along the anticlinal axis which now separates the tertiary basins of London and Hampshire. Admitting that this elevation took place when all the strata were beneath the level of the sea, Mr. Lyell endeavours to show, that the returning waters, when the land was raised to its present position above the sea, would have strewed the debris of the older over the newer formations, as we now find it; while those of the more recent would not cover, except in inconsiderable quantities, the more ancient strata; and that the marked dissimilarity between the diluvium of the Wealds of Kent and Sussex, and that of Hampshire and the neighbourhood of London, may thus be accounted for. As the freshwater formations in Hampshire and the Isle of Wight, as well as the Plastic and London clays, are covered by deep beds of a similar gravel, consisting of chalk-flints, the author states several geological facts to prove, that these more recent formations existed when the chalk and tertiary strata were elevated, and notwithstanding their difference of inclination, even when the strata of Alum-bay assumed their vertical position; and consequently they were all covered indiscriminately by a similar stratum of diluvium.

April 7.—A translation was read of a letter from M. de Gimbernat, of Geneva, principally upon sulphate of soda, to G. B. Greenough, Esq. F.G.S. &c.

A paper, entitled, " On the geology of the valley of the St. Laurence;" by John J. Bigsby, M.G. F.G.S. was read in part.

April 21.—The reading of Dr. Bigsby's paper was continued.

ASTRONOMICAL SOCIETY.

March 10.—A paper was read " On an appearance hitherto unnoticed in the nebula of Orion," communicated by the Astronomer Royal. This appearance was detected by means of Mr. Ramage's 25-feet reflector, which is now placed up at the Royal Observatory. It is well known that among a variety of stars, which appear at the same time in the field of view of the telescope with this nebula, there are four very bright ones, which form a trapezium, and, at a little distance, three others nearly in a straight line. These three stars, Mr. Pond observes, are neither situated on the edge of the nebula, nor are they parallel to the edge; but they seem to be insulated from the nebula, the light of which retires from them in a semicircular form, as if they had either absorbed or repelled the light from their immediate vicinity.

The same appearance, the Astronomer Royal remarks, is observable in the trapezium, round the four stars of which the light has also receded analogously, leaving them on a comparatively dark ground. He conjectures that the *stars* have been the immediate cause of the disappearance of the light; and therefore he wishes to draw the attention of astronomers to the phænomenon, as it seems to deserve a marked attention.

The Astronomer Royal has noticed a similar appearance, still more decidedly, in another part of the same nebula at some minutes distance from the trapezium.

2. There was read a communication from Colonel Mark Beaufoy, a member of the Council of this Society. It contains

1st. Observed transits of the moon and of moon-culminating stars, over the middle wire of his transit instrument at Bushey Heath in Sidereal time. These were observed in the course of 1825, and amount to 322.

2dly. Occultations of stars by the moon, in number 6.

3dly. Observations of two lunar eclipses, in 1825.

4thly. Observations of eclipses of Jupiter's satellites, in 1825, at Bushey Heath. These amount to twenty-five, and the results are given both in Bushey and Greenwich, mean time.

There was also read a communication from Major J. A. Hodgson, of the 61st Bengal Native Infantry, Revenue Surveyor General, residing at Futty Ghur, on the Ganges. This letter records seventy-five observations of the eclipses of Jupiter's satellites, made at Futty Ghur (latitude 27° 21′ 35″ N.) in the autumn of 1824 and spring of 1825. Some of these observations were made by Major Hodgson himself, and others, under his superintendance, by young men who are his apprentices in the Revenue Survey Department. The names of the several observers are given:—each observation has its appropriate meteorological indications registered:—and the natures, powers, and qualities, of the telescopes employed, are respectively described. These observations, compared with corresponding observations of the same phænomena in any places whose longitude have been accurately ascertained, will serve to determine the longitude of Major Hodgson's observatory.

An Address delivered at a special General Meeting of the Astronomical Society of London, on presenting the Gold Medals to J. F. W. Herschel, *Esq.*, J. South, *Esq.*, *and Professor* Struve, *on April* 14, 1826, *by* Francis Baily, *Esq. F.R.S. L.S. & G.S. M.R.I.A. and President of the Society.*

The Members of the Astronomical Society are convened together this evening for the purpose of witnessing the distribution of the Medals, which have this year been awarded by the Council, agreeably to the powers vested in them for that purpose. The subject, which has called for this public expression of their opinion and approbation, is that of *Double Stars;* which has been pursued with uncommon zeal and energy by three distinguished members of your body.

The history of this particular branch of astronomy is but of recent date. For, it cannot be unknown to any of you that this subject occupied a considerable portion of the time and attention of our late illustrious President, Sir William Herschel; and that, in fact, it was he who first directed the attention of astronomers to this important branch of the science; having himself commenced and carried on, with great ability and diligence, a minute survey of the heavens, for the express purpose of detecting those almost imperceptible combinations of stars, which had hitherto escaped the observation of ordinary observers.

Assisted by his own inventive genius, and the labour of his own skilful and unerring hand, he contrived and brought to perfection telescopes of a size which may be truly termed *gigantic*, and possessing powers of vision and penetration far superior to any that had ever yet been used by astronomers: and with which he made those astonishing and remarkable discoveries that have filled the contemplative mind with wonder and admiration.

It did not escape the sagacity of this illustrious astronomer that these important discoveries, which he was the first to disclose to the world, might be made conducive to the investigation of the *parallax of the fixed stars:* a subject which has, from the earliest period, occupied the attention and curiosity of astronomers. And it was, in fact, this consideration that first led him to the pursuit of this important branch of astronomy: but this object was soon lost sight of, in the singular and remarkable phænomena which he afterwards brought to light*.

Before he commenced his observations, however, he was desirous of ascertaining what other astronomers had done before him in the same pursuit. But, not having the facility of reference to many works, he himself (as he emphatically expresses it) opened the Great Book of Nature, and explored that vast and splendid Volume, as the best Catalogue that he could find for the occasion. At the time that he began his important and interesting enquiries, he was not aware of more than *four* stars that came under the description of double stars: yet, with this small stock he began his pursuit; and, in the course of a few years, formed a catalogue of 269 double and triple stars, which he presented to the Royal Society, and which is published in the *Philosophical Transactions* for 1782. In this Memoir, and in all his subsequent ones, he gave not only the *Distances* between the two stars, as measured by various methods, but also the *Angle of Position*, or the angle formed by the parallel of declination, and an imaginary line joining the two stars. These records have now become of considerable importance, as enabling future observers to compare their results, and thus determine the change which those quantities have undergone during the interval that has elapsed since they were made.

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* Indeed the obvious use which might be made of such observations had occurred to Galileo, who first suggested the idea that the apparent distance of two apparently contiguous stars might perceptibly vary according to the position of the earth in its orbit. But, his theory was founded on very imper[illegible] and unsatisfactory data: and he himself made no progress in the solution of this important problem.

Ever ardent in the cause of science, this distinguished astronomer followed up his favourite pursuit by a second collection, consisting of 434 additional double stars; which was published in the *Philosophical Transactions* for 1785.

In the years 1803 and 1804 he communicated to the Royal Society "An account of the changes that have happened during the last 25 years, in the relative situation of double stars:" and it was in these papers that he first made known to the world those astonishing and important facts which have so justly excited the admiration of astronomers. In order to set this in a clearer light, I would remark that it had been hitherto a commonly received opinion, that the difference in the apparent magnitude of the fixed stars was caused by the difference in their distance from the eye of the observer: that a star of the first magnitude, for instance, was situated nearer to us than one of the second magnitude; and this again, nearer to us than one of the third magnitude; and so on in succession till we came to the smallest point visible in the most powerful telescopes: and moreover that those apparent combinations of stars, by twos or by threes, or any larger clusters (numbers of which present themselves to the eye of the observer) were merely the consequence of their lying nearly in the same line of vision, and that they were nevertheless separated from each other by an immense and immeasurable distance. But this, however much it may be true in some particular instances, is not universally the case: for, in the course of the observations alluded to in the two papers just mentioned, the most remarkable and unexpected phænomena presented themselves. The apparent distances of many of the double stars were found to differ from what they had been at a former period; at the same time also that their angles of position were discovered to have undergone a perceptible variation, and evidently indicating a revolution round each other. This was the case whether the star had a considerable proper motion of its own; or whether it was apparently at rest with respect to the other stars around it: thus showing incontestibly that the two stars acted on each other agreeably to the universal law of gravitation.

In fact, in the language of Messrs. Herschel and South, "the existence of binary systems (in which two stars per-"form to each other the office of sun and planet) has been "distinctly proved; and the periods of rotation of more "than one such pair ascertained with something approach-"ing to exactness. The immersions and emersions of stars "behind each other have been noted; and real motions "among them detected, rapid enough to become sensible and "measurable in very short intervals of time." The most remarkable and regular instance of this kind is that of the double star *ξ Ursæ Majoris:* where the stars perform a revolution round each other in the short space of 60 years: and already three fourths of the circuit has been actually observed from the first period of its discovery in 1781 to the present day. The double star *p Ophiuchi* presents also a similar phænomenon, with a motion in its orbit still more rapid. In this case the two stars are very unequal in their magnitude. *Castor*, *γ Virginis*, *ζ Cancri*, *ξ Bootis*, *δ Serpentis* and that remarkable double star 61 *Cygni*, together with several others exhibit likewise the same progressive increase in the angle of position. The instances are indeed too numerous for me to enlarge upon in this place; and I allude to them merely with a view of drawing your attention to this important and interesting branch of the science.

These binary systems, it must be confessed, open a vast field of inquiry and speculation relative to the true system of the universe. The mind is lost in the contemplation of such immense bodies performing their revolutions round each other at such immeasurable distances. Our vast planetary system shrinks to a mere point, when compared to the orbits of these revolving suns. When we consider likewise the remarkable appearances exhibited by clusters of very minute stars, by nebulous stars and by nebulæ, and the singular changes which they seem to be undergoing, and which are too evident to admit of a doubt, and too important to be overlooked, we must confess that there is still much to learn in the science of astronomy.

nomy. It is true that our late illustrious President has drawn some important inferences from those remarkable appearances which he was the first to discover, and has advanced a theory relative to the system of the universe, which whether it be realized or not, (and centuries must elapse before we can even approximate towards the truth of it,) must ever show the vigour of his bold and comprehensive mind.

The last production of this Great Man, relative to double stars, was communicated to this Society, in the year 1821; and is inserted in the first volume of our Memoirs.

Such was the state of this interesting branch of the science at the time it was taken up by Messrs. Herschel and South. The singular and extraordinary changes that had been observed by Sir William Herschel in his review of the heavens in 1802 and 1804, had determined Mr. Herschel to follow up the intentions of his father, by a review of all the double stars inserted in his catalogues: and as early as 1816 he had commenced this arduous undertaking. Mr. South also being disposed to pursue the same enquiry, suggested the plan of carrying on their observations in concert: and, with the aid of two excellent achromatic telescopes, belonging to the latter, they employed the years 1821, 1822, and 1823 in this research. The result of their labours was presented to the Royal Society, and published in the *Philosophical Transactions* for 1824 at the expense of the Board of Longitude.

The number of double stars observed jointly by these two astronomers amounts to 380: and we may judge of their value and importance when we learn that the authors were more anxious to obtain accurate results, than to extend the field of their inquiries in the first instance. But, when we find that, even to obtain these results, many thousand measurements of distance and position were made, we must justly admire the patience and perseverance of the authors in this their laborious, but highly important pursuit. The remarkable phænomena, first brought to light by Sir William Herschel, have been abundantly confirmed; and many new objects pointed out as worthy the attention of future observers.

Whilst these important inquiries were carrying on in England, one of our Associates, Professor Struve, was engaged in similar observations at Dorpat in Russia. The result of his labours is contained in the several volumes of the *Observations* made at that observatory; and will be read with pleasure and advantage by every lover of astronomy*. The remarkable coincidence in most of the measurements made by M. Struve, and those made by Sir William Herschel and afterwards by Messrs. Herschel and South (although with very different instruments and micrometers), confirms the general accuracy of the observations, and marks the degree of confidence that may be placed in measurements of this kind. Some slight discrepancies have indeed been observed on a comparison of the total results, and some singular anomalies have presented themselves: but these, so far from invalidating their accuracy, tend to give them greater confirmation, and may probably, at some future period, lead to the detection of some hidden law which regulates the motions of these remarkable bodies.

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* Although not immediately connected with the object of this Address, I cannot omit this opportunity of noticing the labours of M. Amici on double

It is for these important observations and discoveries, and for the great zeal and talent displayed by these distinguished astronomers, in the pursuit of this interesting subject, that your Council has resolved to bestow on each of them the Gold Medal of the Society: and which I have now the honor of doing.

[The President, then addressing Mr. Herschel, said:] "In "the name of the Astronomical Society of London, I present "to you this Medal. You will accept it, Sir, as a mark of "the deep interest which this Society takes in the object of "your labours. Be assured that we are pleased to see (from "the Paper presented to us this evening) that the subject still "occupies your attention, and that it is likely to be pursued "with so much energy and zeal, by one who can so fully ap"pretiate the importance of such inquiries, and who is so "competent to conduct investigations of this kind. We trust "that you will have health and strength to pursue the path "which you have thus commenced with so much honour to "yourself, and so much benefit to science. Inheriting, as you "do, those rare and exalted talents which distinguished your "venerable and honoured father, and aided by the resources "of your own powerful and enlightened mind, you have al"ready opened another and very interesting field of inquiry "and research in this particular branch of astronomy, by pro"posing *a new method of applying such observations to the* "*investigation of the parallax of the fixed stars:* a subject "which cannot be fully appretiated till after the lapse of many "years, and which we hope will not be lost sight of by those "who are engaged in investigations of this kind. The name

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double stars. With some excellent and beautiful telescopes and micrometers of his own workmanship and construction, this indefatigable and careful observer has extended his examination to upwards of 200 double stars; and has detected motions in some of them, not yet noticed by other astronomers. It is to be hoped that his very valuable labours will be collected and published, for the benefit of science.

“ of Herschel, doubly connected as it thus is, with the history
“ of astronomy, can perish only with all records of the science.
“ The splendid example of the father has been emulated by
“ the son: and you have the proud and enviable satisfaction
“ of knowing that you will share the Glory of his Immortal
“ Name.”

[The President next presented the Medal to Mr. South in a similar manner, and said:] “ In presenting you with this
“ Medal, Sir, I can only repeat the sentiments which I have
“ just delivered to your friend and fellow-labourer Mr. Her-
“ schel. The ardent zeal which you have always evinced in
“ the cause of astronomy, the patience and perseverance
“ which you have shown in conducting so many and so valu-
“ able observations, of no ordinary kind, and the skill and
“ accuracy which you have displayed in those delicate mea-
“ surements, are subjects that are duly estimated by this So-
“ ciety. Possessed of a princely collection of instruments, of
“ exquisite workmanship and considerable magnitude, such
“ as have never yet fallen to the lot of a private individual,
“ you have not suffered them to remain idle in your hands,
“ but have set an example to the world how much may be
“ done by a single person, animated with zeal in the cause of
“ science. Scarcely indeed have those labours issued from the
“ press, for which this Society is now assembled to congra-
“ tulate you, than they have been followed by a communi-
“ cation of others (now lying on the table) rivalling them i
“ magnitude and importance; extending your examination t
“ 460 additional stars (many of which are *new*), and confirmin
“ in a satisfactory manner the remarkable changes which ha
“ been noticed in your previous review. The subject whic
“ you have thus commenced with so much success, with so
“ much benefit to science and so much honour to yourself, i
“ as vast as it is important. The number of double and triple
“ stars seems to increase with the attention that is paid to
“ them: and already their amount is sufficient to appal an
“ ordinary observer. Boldly pursuing the path of science,
“ your energy has, however, increased with your difficulty; so
“ that few of these singular bodies have escaped your patience
“ and penetration: and the Society hope and trust that the
“ same talents will be exerted in a further prosecution of the
“ subject. There is no doubt but that a careful examination
“ and re-examination of these remarkable bodies will tend to
“ throw some new and interesting light on the system of the
“ universe: and it must ever be a pride and satisfaction to
“ you to reflect that you have been instrumental in advancing
“ the boundaries of this department of science, and that your

" own Name will always stand conspicuous in the history of " these discoveries."

[The President afterwards presented the Medal, in a similar manner, to Mr. Herschel, as proxy for Professor Struve, and addressed him as follows:] " Assure M. Struve of the lively " interest which we take in all that is passing at the Observa" tory of Dorpat: that we admire the patience, the exertions " and the address, with which he has overcome the difficul" ties he has had to encounter, in the progress of his disco" veries: and that we look forward with confidence to a con" tinuance of the same brilliant career in the cause of astro" nomy. Furnished, as he now is, with one of Fraunhofer's " colossal telescopes, and thus armed with the most powerful " means, we anticipate the most successful results from his " laborious exertions. Unconscious of what was going forward " in this country, he had opened for himself a vast field of " inquiry, which he has pursued with the most splendid suc" cess; and which places his name amongst the most cele" brated of modern astronomers. The Paper which has been " read to us, this evening, shows that his ardour is unabated: " since he there announces the important fact of the observa" tion of 1000 double stars of the first four classes, (most of " which are entirely new,) and amongst which are 300 of the " first class. To a mind, formed like his for the pursuit of " science, little need be said to animate him to a continuance " of his labours: but, it may be pleasing to him to know that " we are alive to the progress of his discoveries: and I am " sure that you will convey to him, in much better terms than " I can do, the expressions of our esteem and admiration for " his services in the cause of science;—services which assure " us that the name of Struve will be imperishable in the an" nals of astronomy."

ROYAL INSTITUTION OF GREAT BRITAIN.

April 7.—Mr. Faraday spoke in the Lecture-room on the subject of vapour of extreme tenuity, opposing the general opinion that vapour may be diminished in its tension *ad infinitum*, and stating that there was reason to believe that a limit existed, varying with different bodies, but beneath which they gave off no vapour. He began from Dr. Wollaston's argument of the finite extension of the atmosphere, and then showed that either gravity or cohesion were sufficient to overcome a certain degree of elasticity, advancing experiments in illustration of the power of cohesion over vapour. He concluded that some bodies might have their limit of vaporization within the range of temperature which we can command, and even near ordinary temperatures; whilst others, as the earths and some of the metals, are perfectly fixed under common circumstances. The bearing of these opinions upon one of the theories of meteorites was pointed out.

Mr. Cuthbert exhibited his fine American microscope, and his short reflecting telescope in the Library; and several specimens of Mosaic gold were also brought for inspection, by Mr. Parker.

April 14.—Dr. Granville gave a condensed account of his researches into the history and processes of mummification, and illustrated it by his fine specimens, an account of which has already been before the public in our Journal.

April 21.—Dr. Harwood read an essay on the natural history of the Asiatic elephant, including some account of the individual lately existing at Exeter 'Change: a cast of the head of this animal was in the room, with a number of other large and small specimens, and a series of finely coloured drawings.

A specimen of illuminated writing, being the fac simile of a page of a missal, was placed upon the table in the Library.

XLVIII. *Intelligence and Miscellaneous Articles.*

THE NEW EXPEDITION INTO THE INTERIOR OF AFRICA.

DISPATCHES, public and private, have been received from Captains Clapperton and Pearce, dated Badagry Roads, in the Bight of Benin, the 29th of November last. On the evening of that day they were to land at Badagry, where, fortunately, they found Mr. Houtson, a British merchant, well known in that part of the country, who not only arranged for them a safe passage in palanquins, through the king of Badagry's dominions, but agreed to accompany them to the next kingdom, Hio, or Eyo, about five days' journey of twenty-five miles each, and there to settle a palaver with the king of that country, who is in constant communication with Nyffe and other parts of Houssa. From him they learn, that once arrived at Hio, he apprehends there is little reason to fear any check to their future progress. From Hio to Tasso is about nine days' journey, and from Tasso to Nyffe nine days' more; so that the whole distance from the coast to Nyffe is twenty-three days, or about 570 miles. At Whydah they met with a M. de Souza, a Portuguese; and also Mr. James, who makes so remarkable a figure in Mr. Bowditch's book, who both recommended a visit to the king of Dahomey, as the direct road to the Sultan Bello's dominions was through a part of his; and as M. de Souza was most intimate with this sovereign, he offered to accompany any of the gentlemen to his capital, Abomey, to obtain permission for them to pass through his territory: for this purpose Dr. Dickson was dispatched with orders to join the party in the interior. They were all in the best health and in high spirits.

ON THE TEMPERATURE OF MINES. BY M. P. MOYLE, ESQ.

During the last summer and autumn, I repeated most of my former experiments on the water in the old and relinquished mines as before stated (*vide* Annals, vol. v. N. S.), and almost precisely with the same results. Suffice it to say on this head, that the greatest heat found in those collections of water from the depth of 20 to 170 fathoms from the surface, was 55° Fahr. in Relistian mine, in the parish of Gwinear, while the coldest temperature found was 52° at 134 fathoms in Huel Ann, in Wendron.

I conceived that by selecting a stagnant collection of water in a deep part of a mine at work, the temperature of which spot while it was occupied by the workmen was known, might more effectually give us the true temperature of the surrounding strata, than by any other means. I, therefore, selected a *winze*[*] at the 110 fathom level, in Huel Trumpet tin mine, in the parish of Wendron. This winze was sunk between four and five fathoms, when it was found necessary to relinquish it from the water being too quick; and until the 120 fathom level was driven far enough under it to drain it of its water.

A hole was bored in the solid granite at the bottom of this winze two feet deep; a thermometer was put into it, and the hole was soon found to fill with water from a natural infiltration without a drop falling into it from above. As this hole filled with water, the thermometer fell to 56°, but in a few hours it rose to 70°, while the air at the bottom of the winze was 72°. I fastened a line to the thermometer, and allowed it to remain in the hole. The place was now relinquished, and was in the course of a few hours full with water, and great care was taken to prevent any of the water in common to the mine from running into this reservoir. On the following day this water was found at the surface 70°, at two fathoms in depth 68°, and at the bottom 67°: at the expiration of nearly three months, it was thought necessary to examine it again, as the approach of the end of the 120 fathom level might otherwise destroy the opportunity sought. The water was now found at all depths to be 54°. A few weeks after this, the water was found to be sinking, when additional care was taken to prevent any water from falling into the winze; when it had sunk to within two feet of the bottom, the thermometer which was allowed to remain in the hole was suddenly withdrawn, when it was found to be at 54°. Two days after this period, this hole was dry, and showed the temperature of 70°.

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[*] A winze is a small shaft sunk simply from one level to another, often required for ventilation, as well as for the judicious working of a mine.

Not willing to rely too much on this single experiment, I sought another opportunity of repeating it in Huel Vor tin mine, situated in slate. Here a winze similarly circumstanced to the one just related occurred at the 124 fathom level. This winze was sunk just six fathoms before relinquished, at which time the temperature was 75°; but after being filled with water for about two months, the registering thermometer indicated only 56°; and this possibly might be influenced in some measure by its being found impossible wholly to exclude a fall of water running into it from above.

I do flatter myself that these experiments tend much to strengthen my former assertions of the earth in general possessing and preserving the mean annual temperature of the latitude; and although these experiments give a degree or two above this mark, we cannot but suppose the local causes of heat in a mine at full work must tend to influence the results; but it should be observed that it falls far below what we are taught to expect at these depths, by those holding a different opinion from myself.—*Ann. of Phil.*

PHYSIOLOGY OF THE BRAIN:—EXPERIMENTS OF MM. FLOURENS, MAJENDIE, ETC.

Analysis of the Physiological labours of the Royal Academy of Sciences of Paris, for the year 1824, *by M. Le Baron Cuvier.* —We have reported in our analysis for 1822, with the interest which they deserve, the experiments of M. Flourens to determine with more precision the functions proper to each particular part of the brain; and we have seen that the result appears to be, that the brain (*cerebrum*) properly speaking, is the receptacle for the impressions transmitted by the organs of sense; the cerebellum the regulator of locomotion; and the medulla oblongata the agent of muscular irritability; that the tubercula quadrigemina in particular participate in this irritant power of the medulla oblongata, and produce as it does, convulsions when stimulated. The author expected that these properties might contribute toward the solution of a problem in comparative anatomy which had for some time occupied the attention of naturalists, to determine the true nature of the different tubercles which compose the brain of fishes.

We have given an account more than once, and especially in 1820, of the doubts which exist with respect to those two tubercles which are interior to the cerebellum, and are generally hollow, containing in the interior one or two pair of smaller tubercles. These have long been considered to be the true brain, the tubercles which they cover, to be the tubercula quadrigemina, and those placed anterior to them the olfactory tubercles, analogous to those which we find in front of the cerebrum in the rat, mole, and other mammalia.

For some years M. Arasky, and subsequently M. Serres, have come to the conclusion, but from anatomical comparison only, that the anterior tubercles constitute the cerebrum, and that the large hollow pair correspond to the tubercula quadrigemina. It follows from the experiments of M. Flourens made on carps, that irritation of the anterior tubercles, or of the superior part of the hollow tubercles, produces no convulsions, but if the base of the last be pricked, violent spasms are induced; which would also lead us to consider the lesser internal tubercles to be tubercula quadrigemina, as well as the hollow tubercle which incloses them. The removal of the anterior tubercles does not at first perceptibly change the animal's condition or manner; but it appears to move less frequently and not voluntarily; it even appeared to the author, as well as he could judge from the state of restraint in which he was obliged to keep the fish thus mutilated, that it could neither hear nor see. The removal of the hollow tubercles produces a much more decisive effect on the œconomy of the animal; it moves no longer, respires with difficulty, and lies on its back or side. M. Flourens does not hesitate to conclude that it is to the tubercula quadrigemina that these hollow tubercles correspond, and considers that the great influence which they exert on the system of fishes arises from their extraordinary state of development in this class of animals. With respect to the single tubercle which has universally been regarded as cerebellum, it exhibits phænomena similar to those of the cerebellum of quadrupeds and birds. Injury of it does not excite convulsions; when removed the fish can scarcely remain on its belly; it swims in an extraordinary way; and it turns on its centre as birds do who have lost the cerebellum. The protuberances which are placed behind the cerebellum in fishes, from which their 8th pair of nerves appears to originate, remain to be examined; those which in the superior classes afford only doubtful or imperceptible analogies. Irritation of all their parts produces violent convulsions, particularly in the opercula of the gills, which derive their nerves from this source. If they be destroyed, the motions of the opercula are lost, and respiration ceases. The same effect follows from dividing them longitudinally. M. Flourens concludes that the cerebral organ of inspiration is found here, circumscribed, distinct, and developed to a true lobe, while in other animals it is scarcely separated from the mass. Similar phænomena are to be observed in the *Gadus lota*, pike, and eel.

The conclusion to be drawn by the author and those who coincide in his views respecting the hollow tubercles is, that the point in which the brain in fishes most essentially differs from that of other classes, consists in the great development of the part which presides over the respiratory function; which M. Flourens accounts for by the more laborious respiration of aquatic animals, who act on the air through the intervention of water, unlike animals respiring in air which immediately penetrates the lung.

It is thus, says he, that the brain is larger in animals endowed with much intelligence, the cerebellum in birds, which are so much more agile than any other, and that this same cerebellum always disappears in reptiles, sluggish animals, the very name of which implies torpor. The author finally expresses an opinion that the parts which render the animal tenacious of life, and especially the spinal marrow, are with respect to volume in an inverse ratio to those upon which the intellectual functions depend; animals destitute of the means of defence from violence require a blunted or coarse description of vital condition, which should be to them what we might designate a defence against the effects of its own peculiar condition.

M. Flourens being obliged to make so many and such extensive wounds of the brain to resolve questions so important to humanity, took the opportunity of making numerous observations respecting injuries of this organ and the regeneration of its coverings, as also upon the corresponding phænomena in the animal's faculties as the reproductions advance. To analyse these observations made day after day would require a copy of them, and the details would prove equally interesting in this point of view, if our limits permitted us to enumerate them. In general, where a portion is removed, a clot of blood is formed, and a scab produced, beneath which lymph is deposited. The bone exfoliates; beneath this exfoliation and scab a new skin forms which casts them off, and beneath this skin a new bone forms; but this new skin does not consist of true corium or rete mucosum, nor is the bone formed with two laminæ and a diploe. The new skin is continued from the old, and requires for its formation that the lymph from which it is produced should be maintained in its position either by the scab or some other means. The entire portion of brain removed is not regenerated, but a cicatrix is formed upon the cut surface. A simple division is repaired by reunion. The superior part of the ventricle, when removed, is repaired by a production from the margins of the remaining part. Finally, as we have observed in 1822, the animal recovers by little and little its faculties as the parts cicatrize, at least they do so if the injury has not been very great.

M. Majendie has also made many experiments respecting the functions peculiar to the different parts of the brain, and has communicated to the Academy one of the most remarkable, which in every respect corresponds with one made on the cerebellum by M. Flourens, and which serves as a support to it. When the great commissure of the cerebellum (*pons varolii*) is divided anterior to the origin of the 5th pair of nerves, the animal loses all power of supporting itself on its four limbs; it falls on the side upon which the division has been made, and rolls over and over during entire days, ceasing only when prevented by some obstacle. The harmony in the motion of its eyes is also destroyed; the eye of the injured side is irresistibly directed downward, while that of the opposite side is turned upward. A Guinea pig thus treated turns over and over sixty times in a minute. This rotatory movement is produced by division of one of the crura cerebelli, but if both be divided the animal remains without motion; the equilibrium of these two organs being as essential to the repose as to the regular movements of the animal. Similar phænomena are exhibited when the cerebellum itself is divided from above downward. If three quarters of it be left on the left side, and one quarter on the right, the animal turns over to the right, and its eyes are distorted as stated above; a similar section leaving the one quarter on the left side re-establishes the equilibrium, but if leaving the quarter on the right untouched it is cut on the left down to the crus, the animal turns to the left, or in other words it turns to the side where least is left. A vertical section of the cerebellum puts the animal into an extraordinary condition: its eyes appear to project from the orbit; it leans sometimes to one side and sometimes to the other; its limbs are stretched out as if it endeavoured to go backward. M. Majendie quotes an observation of M. Serres, which proves that the same effects might take place in the human subject; an individual after excessive drinking was seized with a propensity to turn over and over, which continued till death; on dissection,

tion a rupture of one of the crura cerebri was discovered. M. Majendie has not confined his observations to the centre of the nervous system, he has made some very curious observations respecting the nerves distributed to the organs of sense. Hitherto the first pair of nerves or olfactory has been considered as dedicated to the organ of smell. M. Majendie, wishing to make an experiment which appeared to him a work of supererogation, to prove the correctness of an opinion doubted by none, cut the olfactory nerves of a young dog. What was his surprise the following day to find the animal sensible to strong odours! The experiment repeated on other animals afforded similar results. The author suspected that this sensibility was to be attributed to the branches of the fifth pair distributed to the nostril; he accomplished the division of these nerves on either side, notwithstanding their depth, in dogs, cats, and Guinea pigs, and thus destroyed all sensibility in the nostril. Animals which sneezed, rubbed the nose, and turned away the head when compelled to inhale the vapour of ammonia or acetic acid, remained passive when the fifth pair was divided, or at least manifested only the effects resulting from stimulation of the larynx. This effect of strong odours remained even in hens, from whose heads the whole cerebral hemispheres and olfactory nerves had been removed. We might certainly suspect that the volatile alkali acted only chemically on the pituitary membrane, and attribute the effects more to pain than smell; in that case the pain alone would depend upon the fifth pair: but M. Majendie, who saw the force of this objection, observes, that it is much weaker with reference to the animal oil of Dippel or essential oil of almonds, which affected the organ before the fifth pair was divided, and lost all effect when it was cut, although the first pair remained untouched. What would still better rebut the objection, would be to prove that animals which have had the olfactory nerve divided, still continued to seek and distinguish their food by the nose. The experiments on this head do not appear as yet conclusive, but he promises to prosecute the investigation. The dissections of Dr. Ramond, reported by M. Majendie, prove also that when the hemispheres are gorged with blood, or that deep and rooted alteration's take place in their cortical substance, the sensibility of the nostril to the most delicate odours is not impaired. But it is not to the sense of smell alone that the participation of the fifth pair is essential; it contributes to all the senses of those organs to which it is distributed; when divided, the sense of touch is also destroyed, but on the anterior part of the head only; behind the ear and on

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the back of the head it is unimpaired as in other parts of the body. The most irritating chemical agents will not produce tears; the eyelids and iris become immoveable; one might even suppose the eye to be artificial. After some time the cornea becomes white and opaque, the conjunctiva and iris inflame and suppurate, and finally, the eye shrinks into a small tubercle, which fills only a small part of the orbit, and its substance resembles newly coagulated milk. In this state the animal is no longer guided by its whiskers, as it should if merely deprived of sight; it advances with the chin resting on the ground, and pushing its head before it; the tongue is equally insensible, and hangs out of the mouth; sapid bodies appear to have no apparent effect on its anterior part, although they exert their influence on its centre and base. The epidermis of the mouth thickens and the gums separate from the teeth. The author even thinks, that he has observed that the sense of hearing is lost by the division of the fifth pair, which if correct, shows that all the senses are under the influence of this nerve. It has long been known that it was in the lingual branch of the fifth pair that the sense of taste essentially resided, and more recently the experiments of Mr. Bell prove, that the sensibility of the face depends upon the numerous branches of this nerve distributed upon it, but those distributed to the nose, eye, and ear, were not considered equally essential to the integrity, or even to the perfect exercise, of the senses of smell, sight, and hearing, as has been shown by M. Majendie. The details of these experiments, and of others not less interesting, may be found in a journal of physiology, of which the author publishes four numbers in the year, and where he collects whatever is founded on positive facts, established by accurate observations.

M. Flourens has also endeavoured to apply his method of successive removal to determine the use of the different parts of the ear. We know that this complicated organ is composed in warm-blooded animals of an external passage leading to the membrane of the tympanum, which forms the entrance into a second cavity named tympanum or box, and from which a chain of bones commences, the last of which, the stapes, is applied to the fenestra ovalis, or entrance of the second cavity called vestibule, into which three canals called semicircular canals and one of the orifices of a third cavity of a spiral form called cochlea open, the other orifice of the cochlea opening immediately into the tympanum by the fenestra rotunda. There are also mastoid cells formed in the substance of the bone, which communicate with the tympanum, and a canal called the fallopian tube which leads from the tympanum to the back of the nostril or fauces. M. Flourens in a previous investigation, endeavoured to ascertain what part of the organ of hearing should be considered most essential to the perfection of the sense. Pigeons were made the subject of experiment; birds having the ear enveloped in a delicate cellular structure easily removed. He destroyed the meatus auditorius, the first bones, and the tympanum, without destroying the sense; the stapes was then removed, and hearing was sensibly injured; merely raising this bone from its situation, and then replacing it alternately diminished and re-established the faculty; on removing the semicircular canals much more remarkable phænomena were observed; not only the animal continued to hear, but the impression of sound became painful, the slightest noise produced severe agitation, and its head was moved horizontally from right to left with remarkable violence, which did not cease till perfect rest was obtained, and re-commenced when the animal attempted to move. Exposure of the vestibule, and destruction of part of the nervous pulp contained within it, did not entirely destroy hearing: to effect this, the total removal of the whole of the pulp and the nervous expansions continuous with it was necessary, the animal remaining deaf although the rest of the ear was untouched. The author concludes, that the pulp in the vestibule is the essential part of the organ, and that it is in fact, as shown by Scarpa and Cuvier, the only part existing in inferior animals; so that we may consider *the other parts of the organ as serving to give to this sense the different degrees of perfection, which characterize it* in the higher classes of animals.

We have given the above report at full length, not so much on account of the value of the information communicated, as to put our readers in possession of the opinion entertained by the highest literary tribunal in France respecting those experiments which have latterly so much attracted the attention of physiologists. We do not however, by any means, consider that those experimenters have settled the respective questions which they profess to decide, but look upon their labours as little more than so much argument in favour of pursuing the investigation; in which light it is to be hoped that the authors themselves view the subject. With respect to the experiments of M. Majendie, to determine the nerve to which we are indebted for the sense of smell, they must be admitted to be inconclusive, if not altogether fallacious, as we hope to be able to demonstrate in another place.—*Dublin Phil. Journ.*

ACTION OF LIME ON ALCOHOL.

It was known that when alcohol and lime are kept in contact, during a length of time, the alcohol becomes pale yellow[*]. Dr. Menici introduced into a vessel, three ounces of alcohol, of 35 degrees (B), and a similar quantity at 28, into another; each vessel containing also an equal quantity of lime; and they were all exposed to the ordinary temperature, being previously well closed. At the end of four months, the liquor in the second vessel had become sensibly yellow, which soon became deeper, and in six months it was reddish. The alcohol now restored reddened litmus, owing to the solution of lime. Submitted to distillation it afforded unaltered alcohol. The residual liquor, evaporated to dryness, afforded a substance analogous to our black resin [*colofonia rossastra*], which, when kindled, burned brilliantly, with much smoke[†]. But the strong alcohol contained in the first bottle seemed not to have been, in any manner, affected; unless that it feebly restored the colour of reddened litmus.—(*Giornale di Fisica.*) *Dublin Phil. Journ.*

MR. DALTON ON THE CONSTITUTION OF THE ATMOSPHERE.

The following is an abstract of Mr. Dalton's paper on the constitution of the atmosphere, read before the Royal Society on the 23d of February last.

After some preliminary remarks, the author observes, that whatever may be thought of Newton's hypothesis as to elastic fluids, as far as the *mechanical* effects of such fluids are objects of inquiry, we may safely adopt it; namely, that *each fluid is constituted of particles repelling one another by forces inversely as their central distances*, at least within ordinary limits of condensation and rarefaction.

After adverting to the fact that mixtures of various elastic fluids, such as is the atmosphere, composed of atoms of different volumes and elasticities, do notwithstanding observe the same laws of condensation and rarefaction as simple elastic fluids, and to the difficulties which this fact throws in the way of the Newtonian hypothesis, Mr. D. puts a case which he thinks has not before been considered, and which may assist us materially in forming a correct notion of such mixed atmospheres.

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[*] Gay-Lussac first noticed this change of colour, while engaged in distilling alcohol off lime, which he proposed as a better method of obtaining alcohol than by means of muriate of lime. [*Memoires d'Arcueil*, tome iii. p. 104.] But this method had been practised long before. See *Elémens de Pharmacie*, par Baumé, 1770, p. 474.—DUBLIN EDIT.

[†] When sulphuric acid and alcohol are distilled, as in the preparation of æther, towards the end of the process the mixture becomes black, and a black matter collects (if the quantity operated on be large) into a mass. This black mass is brittle; it may be melted; it solidifies on cooling: it is combustible, and burns with a smoky flame. It is, in fact, a kind of pitchy matter, which seems very much to resemble this resin noticed by Dr. Menici.—DUBLIN EDIT.

Two equal cylindrical pipes are conceived to be placed perpendicular to the horizon, in contact, and of indefinite length, close at the bottom, and open at the top. These are supposed to be filled with two gases of different kinds, the one with carbonic acid, and the other with hydrogen, in order to show the contrast more strikingly. The columns of gases are assumed each to be of the weight of 30 inches of mercury, and consequently will represent vertical columns of atmospheres of the respective gases equal in weight to like columns of the earth's atmosphere. Mr. D. calculates from known principles that the column of carbonic acid gas will terminate at 30 or 40 miles of elevation, or at least will become of such tenuity as that it may be disregarded. In like manner that of hydrogen will be found to become insignificant above 1200 miles of altitude. The author then supposes that horizontal air-tight partitions are made across both tubes at any given intervals of distance, and that openings are made, so that the gases in the corresponding horizontal cells may communicate with each other; in which case each gas, as is well known, would divide itself equally between the two cells. For 30 or 40 miles both gases would be found in each cell; but for the rest of the column, namely, for 1000 miles or upwards, there would be nothing but hydrogen in both cells.

In the next place, Mr. D. conceives the horizontal partitions to be withdrawn, and considers what change would ensue. There would have been many cells about the summit of the carbonic acid atmosphere which, when opened for the purpose of communication, would part with half their contents to the collateral cells, but *half* the contents would not be able to fill the *whole space* of the cell, by reason that the gas was at its minimum density before. Hence the gas would be confined to the lower half of the cells, and there would be no carbonic acid in the upper parts. Of course when the partitions were removed, the carbonic acid in each cell would descend till it came in contact with the like gas of the inferior cell. Thus there would be a slight descent of the upper regions of carbonic acid gas. The same also would happen to the hydrogen gas about the summit of its atmosphere, and a still more considerable descent would take place. Mr. D. seems to think there would be no material change in the mixed atmospheres afterwards. Thus the two mixed atmospheres would exhibit equal *volumes* of each gas in the lowest cells, or at the surface of the earth, though in the whole compound atmosphere the two gases are of equal *weights*.

All this would take place according to the author's arguments were the mixed atmospheres *quiescent;* but if the atmospheres are like the earth's atmosphere, in a constant state of commotion, greater or less, still there will be a constant tendency towards that state of equilibrium which is above described. In the conclusion Mr. D. states, that he has a series of observations which support the opinion that the atmosphere at different seasons and elevations exhibits different proportions of its elements in association, which he intends to bring forward on some future occasion.—*Annals of Philosophy.*

ANALYSIS OF OIL OF WINE, &c.

On the 9th of March, a paper on this subject and on the sulphovinates, by Mr. H. Hennell, of Apothecaries' Hall, was read before the Royal Society:—the following is a summary of its contents.

Mr. Hennell at first supposing that the elements of oil of wine were the same as those of sulphuric æther, endeavoured accordingly to determine their relative proportions in the former substance, by passing its vapour over ignited peroxide of copper. In this process, portions of sulphurous acid gas and sulphate of copper were invariably obtained: in attempting to ascertain the origin of which, the oil of wine was heated in solution of muriate of barytes, but no precipitate or even cloudiness was produced in it, though litmus paper at the same time indicated the presence of free acid. On concentrating the solution, however, a precipitate of sulphate of barytes was gradually formed; showing that either the sulphuric acid was in some state of combination interfering with its action upon tests, or that its elements existed in the oil of wine in some unusual state of arrangement. From 200 grains of pure oil of wine, treated with solution of potash, evaporated to dryness and ignited, and then treated successively with nitric acid and muriate of barytes, were obtained 218·3 of sulphate of barytes, indicating 74 of sulphuric acid.

On resuming the analysis with peroxide of copper, with due care, and the additional precautions suggested by the nature of the substance as just pointed out, it appeared that 100 grains of oil of wine contain 53·70 of carbon, and 8·30 of hydrogen: the deficiency = 38 parts being referable to the sulphuric acid, as shown by the experiments above mentioned. These proportions indicate the hydrocarbon combined with the sulphuric acid to contain an atom of each constituent; but they do not show the quantity of hydrocarbon combined with the sulphuric acid, for oil of wine always holds in solution an excess of this hydrocarbon, from which it is impossible to free it. In order to determine, therefore, the quantity of hydrocarbon in *combination* with the sulphuric acid, some oil of wine was heated with water, and precipitated carbonate of barytes was then added to it, which was dissolved, with effervescence. When, however, the solution was evaporated, it soon became acid, and sulphate of barytes precipitated. On treating a further quantity of oil of wine in the same manner, but precipitating the barytic solution by carbonate of potash, and evaporating at a temperature of 150° Fahr. it yielded tabular crystals, not unlike chlorate of potash, very soluble in water and alcohol, and burning with a flame resembling that of æther. These crystals were found to contain, in 100 parts,

Potash	28·84
Sulphuric acid	48·84
Carbon	13·98
Hydrogen	2·34
Water	7·00
	101·00

It thus appears, that in this salt four proportionals of carbon united with four of hydrogen, are combined with one of sulphuric acid, forming oil of wine.

Mr. Hennell ascertained that this salt was identical with that called sulphovinate of potash; and whilst preparing some of the sulphovinates, for the purpose of comparing them with the salts obtained from oil of wine in this manner, he found that a great reduction of the saturating power of sulphuric acid was produced by its mixture with alcohol; 440 grs. of acid mixed with an equal weight of alcohol, requiring for their saturation only 398 grs. of partially dried carbonate of soda; whilst an equal weight of pure acid required 555 grs. of the same carbonate. This fact shows that sulphuric acid, by mixture with alcohol, is immediately converted into sulphovinic acid; and, in conjunction with the facts detailed in the former part of the paper, it also evinces that the loss of saturating power cannot be owing, as MM. Vogel and Gay-Lussac have supposed, to the formation of hyposulphuric acid.

By heating oil of wine either in solution of potash, or in water, much of the excess of hydrocarbon which it contains is liberated in the form of an oil, resembling in appearance some of the balsams. This oil, as well as the crystals which form spontaneously in oil of wine, yielded by analysis carbon and hydrogen, in proportions nearly approximating to those of olefiant gas; but in the analyses, which were several times repeated, a slight loss was always experienced, the cause of which Mr. Hennell was unable to ascertain.—*Ann. of Phil.*

MECHANICAL NOTATION OF MACHINERY.

A paper was lately read before the Royal Society, On the expression of the parts of machinery by signs; by C. Babbage, Esq. F.R.S. of which the following is a notice.

In contriving his calculating engine *, Mr. Babbage found great difficulty from not having any regular method, by which he could find, at an instant's notice, the precise time at which any given piece began to move, and also the state of motion or rest, at the same instant, of all the other parts. He therefore devised a method of expressing all the motions of any machine, however complicated, by signs. This it is almost impossible to describe without figures; but the following statement of the information which may be derived, almost at a glance of the eye, from the paper on which the "mechanical notation" of any machine is expressed, will serve to show the important purposes to which the method may be applied.

1. The name of each part is written at length, and there are references from the name to all the drawings.

2. The number of teeth on each wheel, pinion, rack, or sector, is seen.

3. Any given part, a wheel for example, being named, it will be seen what immediately moves it, what drives the mover, and so on up to the origin of motion: and not only will the whole succession of movements be visible, but the manner in which they act; as, for instance, whether by being permanently connected, or in the manner of a pinion driving a wheel, or by stiff friction, or at intervals only.

4. The angular velocity of each part will be seen.

5. The comparative angular velocity, or the mean velocity.

6. All parts which require adjustment will appear; and the order in which those adjustments should be made is pointed out.

7. At any part of the cycle of the engine's motion, it will be seen at a glance what parts are moving, what are at rest; and it will appear in what direction the motions of the moving parts take place, and whether their velocity is uniform or variable. It will also be seen whether any given bolt or click is locked or not.

8. Any part being named, the entire succession of its motions and intervals of rest is at once presented to the eye; and if the contemporary movements at any particular time be required, they will be visible adjacent to it.

Mr. Babbage gives, as specimens of his method, the mechanical notation of the common eight-day clock, and of the hydraulic ram.—*Ann. of Phil.*

Results of the Meteorological Tables at the end of the Philosophical Magazine, from the 25th of December 1824 *to the* 25*th of December* 1825.

By William Burney, *LL.D.*

1825.	Gosport, at half-past 8 o'clock A.M.						London.		Boston.		London.	Boston.
	Barometer in Inches, &c.	Thermometer.	Temperature of Sp. Water.	Hygrometer.	Evaporation in Inches, &c.	Rain in Inches, &c.	Barometer at 1 P.M.	Thermometer at 8 A.M.	Barometer at 8¼ A.M.	Thermometer at 8½ A.M.	Rain in Inches, &c.	Rain in Inches, &c.
	In.	°	°	°	In.	In.	In.	°	In.	°	In.	In.
January ...	30·138	41·55	50·99	76·7	0·82	1·960	30·183	39·42	29·925	38·69	1·10	0·81
February ...	30·187	40·97	49·82	75·2	1·00	0·825	30·260	37·74	30·025	37·64	0·80	0·60
March	30·021	41·07	49·06	69·8	2·06	2·915	30·108	37·11	29·931	37·61	1·09	1·85
April.........	30·071	49·36	48·96	60·4	4·88	1·255	30·130	44·19	29·876	46·58	0·80	1·55
May	29·880	57·07	49·34	62·7	4·45	2·780	29·920	51·80	29·612	53·78	2·65	3·00
June........	30·066	61·09	50·31	54·1	7·70	2·135	30·048	54·81	29·659	58·27	0·55	1·68
July	30·075	65·77	51·43	54·3	8·87	0·440	30·095	60·53	29·622	63·17	0·12	0·97
August......	29·963	64·29	53·05	60·9	6·80	2·030	29·976	59·97	29·482	62·26	1·30	1·88
September..	29·891	63·55	54·39	71·3	4·30	3·280	29·919	60·45	29·432	62·71	3·05	1·00
October.....	29·987	55·17	55·01	74·7	3·15	2·990	30·021	51·67	29·566	53·68	2·50	2·05
November...	29·817	44·10	54·26	80·2	1·56	3·575	29·722	42·10	29·499	41·00	2·61	2·14
December...	29·535	43·40	52·18	88·6	0·97	6·465	29·583	43·85	29·253	42·87	5·17	2·89
Averages for 1825.	29·969	52·28	51·56	69·1	46·56	30·650	29·997	48·63	29·657	49·85	21·74	20·42
Averages for 1824.	29·854	51·05	50·65	66·6	32·78	40.664	29·880	47·56	29·579	48·44	30·25	28·33

In order to obtain the correct mean annual results of the barometers, thermometers, and depths of rain at Gosport, in London, and at Boston for this table, I recalculated the tables at the end of the Numbers of the Philosophical Magazine and Journal for 1825.

I shall here notice by the way of *Errata*, that Mr. Veall's barometer appears too low by 45-100dths of an inch on the 11th of May, and on the 16th of that month 5-10ths of an inch too low*. Again, on the 26th of November, Mr. J. Cary's barometer is too high by 6-10ths of an inch. These errors I have corrected in the monthly mean pressures in the table, as on comparison it will be readily discovered that they are errors by some means or other.

The mean annual heights of the barometers in the table at the different stations this year, will be found much higher than they were last year, particularly at Gosport and in London: and the mean annual temperatures of the external air are more than a degree higher. The aggregate depth of rain at each place is nearly one-third less this year than last. I am much disappointed at the discontinuation of the use of the pluviameter in London; but by the way of making the table complete, I have substituted the depth of rain that fell there in November and December by approximation. As the varieties of weather, and the heights of barometers very much depend on the position of the prevailing winds, as well as on the vicissitudes of the seasons, I think it necessary to notice some peculiarities in their position at Boston and Gosport.

The winds from the North-east and East frequently travel over the Russian empire, Denmark, &c., and those from the South-east over part of Asia, Turkey, Hungary, and Germany before they arrive at Boston; and in these directions overland they become drier than the opposite winds which travel over a great extent of sea: hence it is that the pressure at Boston is comparatively greater with these winds than with those from opposite points of the compass.

In comparing the position of the winds as registered at these places, they will seldom be found to blow simultaneously from the same point, and their directions are very often four, sometimes eight points different, and not unfrequently in opposite directions. The difference in their directions at the same time of registering, no doubt arises chiefly from the different latitudes of these places, as it respects a tract of land upwards of two degrees in extent between them; and the South-west and West winds, which are so prevalent here from the Atlantic

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* We have just learnt, however, that Mr. Veall finds these heights to be correct, according to his journal.—Edit.

Ocean, either often die away, or change their direction, before they arrive at Boston, from their meeting with other currents over the land; consequently, a less quantity of rain falls annually at Boston than at Gosport.

MELAINA.

Sig. Bizio considers the black matter of the ink of the cuttles fish as a substance *sui generis*, which he calls Melaina, from μέλας and ἀεὶ. It is obtained by digesting the ink with very dilute nitric acid until it become yellowish, washing it well, and separating it by the filter; it is then to be frequently boiled in water, one of the washings to be a little alkalized, and finally with distilled water.

The melaina is a tasteless black powder, insoluble in alcohol, æther, and water while cold, but soluble in hot water; the solution is black. Caustic alkalies form with it a solution even in the cold, from which the mineral acids precipitate it unchanged. It contains much azote. It dissolves in and decomposes sulphuric acid. It easily kindles at the flame of a candle. It has been found to succeed as a pigment, in some respects better than China ink.—(*Giornale di Fisica.*) *Dublin Phil. Journ.*

LIST OF NEW PATENTS.

To John Bellingham, of Norfolk-street, Strand, for improvements in the construction of cooking apparatus.—Dated 18th of April 1826.—2 months allowed to enrol specification.

To James Rowbotham, of Great Surrey-street, Blackfriars Road, hat manufacturer, and Robert Lloyd, of No. 71, Strand, in the county of Middlesex, for a method of preparing a substance for the purpose of being made into hats, bonnets, coats, and wearing apparel in general, and various other purposes.—18th of April.—6 months.

Results of a Meteorological Journal for March 1826, kept at the Observatory of the Royal Academy, Gosport, Hants.

General Observations.

The first part of this month was alternately wet and dry, but mild for March; the latter part was dry, windy, and very cold.

From the vernal equinox to the end of the month, with the exception of one day, the temperature of the air decreased considerably, with smart frosty nights; and a heavy equinoctial gale blew seven days and nights from the North and Northeast. The 23d was a cold winter-like day, with snow from 9 till 11 A.M.; but from the dampness of the air it was not adhesive to the trees or to the ground, and was the first we had had here during the past winter: it again snowed in the night, and by the morning it had covered Portsdown Hill. Snow also fell here on the 26th, which was the coldest day and night since the 28th of last January. Heavy snow-showers and boisterous winds were also experienced in other parts of the country, particularly to the northward. Early in the morning of the 27th, the ice was one-third of an inch thick, and in the mornings of the 30th and 31st, it was one-eighth of an inch thick. This ungenial weather was a seasonable check upon the budding of the fruit-trees, and has therefore made the spring rather backward; but this will no doubt be beneficial in the end. An early spring, with variable weather, is much dreaded in this latitude, as the frosty nights which almost invariably ensue, have a destructive effect upon the young fruit, and vegetation. The mean temperature of the external air this month, is one-third of a degree less than that of last month! The *maximum* temperature occurred in the night of the 6th, instead of in the day. Spring water seems to have arrived at its *minimum* temperature, as it is now at a stand.

On the morning of the 31st two beautiful *parhelia*, and a fine solar *halo* appeared between 8 and 9 o'clock. The first parhelion on the south side of the sun was visible from eight till half-past, one degree without the exterior colour of the solar halo, and 23 degrees distant from the sun's centre: it varied in shape, being sometimes circular, at other times gibbous and oblong, according to the motion and density of the almost invisible vapour in which it was formed by the reflected rays of the sun; and the orange, light yellow, and blue colours with which it was embellished, were sufficiently vivid to be traced through a passing attenuated *cirrostratus* cloud. The other parhelion on the north side of the sun, which appeared from half-past eight till a quarter to nine, was not so bright in its primitive colours, in consequence of the most dense part of the vapour having passed off by means of a fresh wind from the North-west; but its distance was the same from the sun's centre, viz. 23 degrees. The solar halo was well-defined, its horizontal diameter was 44 degrees, and its whole area presented a lake colour bounded by a turbid red, whilst that part of the sky in its vicinity was gray.

The atmospheric and meteoric phænomena that have come within our observations, this month, are two parhelia, two solar and two lunar halos, three meteors, one rainbow, and thirteen gales of wind, or days on which they have prevailed, namely, one from the North, seven from North-east, one from South-east, and four from the South-west.

Numerical Results for the Month.

		Inches.	
Barometer	Maximum	30·36,	March 31st—Wind N.W.
	Minimum	29·37,	Ditto 24th—Wind N.E.

Range of the mercury . . 0·99.

	Inches.
Mean barometrical pressure for the month	29·958
——— for the lunar period ending the 8th inst.. .	29·971
——— for 14 days, with the Moon in North declin.	29·902
——— for 15 days, with the Moon in South declin.	30·040
Spaces described by the rising and falling of the mercury	6·110
Greatest variation in 24 hours	0·470
Number of changes	23·

Thermometer	Maximum	59°,	March 9th.—Wind S.E.
	Minimum	31	Ditto 26th—Wind NE.

Range	28
Mean temp. of the external air	45·56
——— for 30 days with the Sun in Pisces	47·38
Greatest variation in 24 hours	21·00
Mean temp. of spring water at 8 o'clock A.M. . . .	49·44

DE LUC'S *Whalebone Hygrometer.*

	Degrees.
Greatest humidity of the air .	95 in the evening of the 6th.
Greatest dryness of ditto . . .	50 several times.
Range of the index	45
Mean at 2 o'clock P.M. . . .	64·2
—— at 8 o'clock A.M. . . .	72·3
—— at 8 o'clock P.M. . . .	71·6
—— of three observations each day at 8, 2, and 8 o'clock	69·4

Evaporation for the month	3·520 inch.
Rain in the pluviameter near the ground .	2·615
Rain in ditto 23 feet high	2·370

Prevailing wind, N.E.

Summary of the Weather.

A clear sky, 5; fine, with various modifications of clouds, 13; an overcast sky without rain, 9; rain, 4.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
13	5	24	1	17	23	15

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
3	10	2	3	0	6	2½	4½	31

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr. Burney at Gosport, Mr. J. Cary in London, and Mr. Veall at Boston.*

	Gosport, at half-past Eight o'Clock, A.M.					Clouds.									Height of Barometer, in Inches, &c.		Thermometer				Rain.		Weather.		
Days of Month, 1826.	Barom. Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Evaporation.	Rain near the ground.	Lond. 1 P.M.	Bost. 8½ A.M.	London. 8 A.M.	London. Noon.	London. 11 P.M.	Boston. 8½ A.M.	London.	Boston.	London.	Boston.	Wind
☾ Mar. 1	30·10	48	50·30	75	SW.	...	...	1	...	...	1	1	...	0·470	30·06	29·63	46	47	48	48	...	...	Fair	Cloudy	W.
2	29·78	50	......	80	SW.	...	...	...	...	...	1	1	...	·270	29·90	29·33	47	51	48	47·5	...	·42	Showers	Rain	W.
3	29·66	50	......	87	NW.	1	...	1	...	1	1	1	0·15	·090	29·80	29·38	49	48	47	46	...	·10	Rain	Rain	W.
4	29·64	50	......	88	SW.	1	...	1	...	1	...	1	...	·035	29·74	29·30	49	55	45	44	...	...	Fair	Rain	calm
5	[illegible]	45	......	80	W.	1	...	1	...	1	1	1	...	·680	29·94	29·45	44	48	37	40·5	...	...	Fair	Fine	W.
6	30·06	42	49·60	78	SE.	...	...	1	...	...	...	1	·12	·115	30·00	29·78	38	45	47	37	...	...	Cloudy	Cloudy	W.
7	29·82	[illegible]	......	94	SW.	...	...	1	...	...	1	1	...	......	29·88	29·38	50	55	49	51	...	...	Cloudy	Rain	SW.
8	30·00	[illegible]	......	75	SE.	1	1	1	...	...	1	1	...	......	30·09	29·72	49	55	49	46	...	·18	Fair	Cloudy	W.
9	30·08	51	......	76	SE.	1	...	1	...	...	...	...	·20	......	30·17	29·77	49	60	56	54	...	...	Fair	Fine	calm
10	30·25	51	......	75	E.	1	1	...	...	...	...	...	...	......	30·33	29·95	50	67	45	52·5	...	...	Fair	Fine, 2*p.m.* Ther. 68	calm
11	30·25	47	......	68	SE.	...	...	...	...	...	...	...	...	......	30·33	30·00	44	50	43	48·5	...	...	Fair	Fine	SW.
12	30·31	44	......	62	E.	1	...	...	...	...	...	...	·70	·120	30·47	30·20	44	49	38	44·5	...	...	Fair	Fine	E.
13	30·34	41	49·50	70	NE.	...	...	1	...	...	...	1	...	·480	30·44	30·15	39	45	38	43	...	...	Fair	Fine	E.
14	29·97	45	......	69	SW.	1	1	1	...	1	1	1	...	·020	30·04	29·70	42	50	42	43	...	...	Fair	Rain	SE.
15	29·88	47	......	78	NW.	1	1	1	...	1	1	1	·35	......	29·91	29·48	43	52	38	45	...	·05	Fair	Fine	W.
☽ 16	30·10	42	......	71	NE.	1	...	1	...	...	1	...	...	......	30·25	29·87	35	44	36	38	...	...	Cloudy	Cloudy	NW.
17	30·33	41	......	64	NE.	...	...	1	...	1	...	...	...	·085	30·40	30·15	35	45	34	41·5	...	...	Cloudy	Fine	calm
18	30·25	40	......	70	N.	...	...	1	...	1	1	1	·25	·010	30·12	29·95	33	47	37	36	...	...	Fair	Fine	W.
19	29·93	43	......	68	NW.	...	...	...	...	1	1	1	...	......	29·92	29·53	42	44	38	43·5	...	...	Hail	Fine	NW.
20	30·03	43	49·25	68	N.	...	...	1	...	1	1	...	...	......	29·99	29·50	43	46	38	43	...	...	Showers	Fine	calm
21	29·95	43	......	66	NE.	1	...	1	...	1	1	...	·35	......	30·03	29·82	38	46	38	43	...	...	Cloudy	Rain	E.
22	29·82	41	......	68	NE.	...	...	...	...	...	1	...	...	·240	29·83	29·65	37	41	38	42	...	·8	Cloudy	Cloudy	E.
○ 23	29·64	40	......	[illegible]	N.	...	...	1	...	...	1	1	...	......	29·60	29·42	38	36	34	38	...	...	Rain, &c.	Cloudy. Rain *p.m.*	NW.
24	29·37	41	......	74	NE.	...	...	1	...	...	1	...	·20	......	29·61	29·40	36	39	34	40·5	...	·7	Cloudy	Cloudy. Rain &hail	SE.
25	29·66	40	......	68	NE.	...	...	1	...	1	1	...	...	......	29·70	29·60	35	42	34	40	...	·9	Fine	Rain [*p.m.*	E.
26	29·63	38	49·00	68	NE.	...	...	1	...	1	1	1	...	......	29·81	29·65	38	40	30	35	...	·07	Snow	Rain & snow	E.
27	29·90	35	......	64	NE.	1	...	1	...	1	1	...	·40	......	30·00	29·73	33	41	34	40	...	·09	Fair	Fine	E.
28	29·84	41	......	67	W.	...	...	1	...	1	1	...	...	......	29·75	29·56	39	49	39	42·5	...	...	Fair	Fine. Rain *p.m.*	S.
29	29·64	49	......	70	W.	...	...	1	...	1	1	...	...	......	29·70	29·25	38	44	34	43	...	·04	Cloudy	Cloudy	NW.
☾ 30	30·05	39	......	64	NW.	...	...	...	...	1	1	...	...	......	30·03	29·70	37	46	34	38	...	...	Fair	Stormy	NW.
31	30·30	39	49·00	64	NW.	1	1	1	1	1	1	...	·70	......	30·34	30·04	34	46	38	40	...	...	Fair	Fine	NW.
Average.	29·949	44·16	49·44	72·3		13	5	24	1	17	23	15	3·52	2·615	30·01	29·68	41	47	40	43·0					

THE

PHILOSOPHICAL MAGAZINE
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31st MAY 1826.

XLIX. *On Mr.* Dalton's *Speculations respecting the Mixture of Gases, the Constitution of the Atmosphere, &c. By* Thomas Tredgold, *Esq.**

IT appears that Mr. Dalton's speculations respecting the mixture of gases and vapours, and the nature of the atmosphere, have been very generally received as true explanations of the phænomena of the one and of the nature of the other; and by those who are considered of high authority in science.

Under these circumstances, it becomes the duty of those who reject these speculations as erroneous, to exhibit the grounds on which they do object to them, in the hope that the true explanation of these important points of physical science may be established.

We owe much to Mr. Dalton, even in cases where he has not been successful, and his name will always be respected by those who feel any interest in the progress of knowledge; and I am sorry that I have to oppose as inaccurate one of those bold speculations on which much of his fame has been raised.

If his had been merely speculations, and without influence on the progress of other branches of physical inquiry, they might have remained unopposed; but when formulæ for the reduction of chemical experiments to a common standard are founded on them, and they are made the basis of other theories, and are used in the correction of barometrical measurements, and in various meteorological inquiries, it becomes a work of necessity to examine how far these doctrines are founded in truth.

When Mr. Dalton's opinions first appeared, they were opposed by Mr. Gough, and with sufficient force to have called for more accurate investigation before they were acceded to. Mr. Gough's paper was however not satisfactory to me; and as far as I can recollect, it was very diffuse.

The whole of Mr. Dalton's theory rests upon a very important proposition in aërostatics: for if this proposition be true, the whole of his speculations are at variance from it, and must, therefore, be erroneous. Consequently, the labour of refuting them is reduced into a very narrow compass.

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* Communicated by the Author.

Proposition I.—If an uniform mixture of gases or vapours, which mix without condensation, be confined in a close vessel, the elastic force of each gas on a given surface must be the same, and equal to the elastic force of the mixture on the same extent of surface.

Let p be the elastic force of the mixture, and V the volume of the vessel. Also let A and B be the two gases, and v the volume of the gas A when its elastic force is p.

It is obvious, that v must be less than V, otherwise the gas A would entirely fill the vessel, and a mixture could not be formed without condensation.

But since v is less than V, and the gas A is uniformly distributed throughout the greater volume of the vessel V, its parts must be kept asunder by a force which is not less than its own elastic force; and as the force which keeps separate the parts of the gas A, is the elastic force of the gas B, therefore, the elastic force of the gas B in the mixture cannot be less than that of A.

But by the same steps it may be proved that the elastic force of the gas A cannot be less than that of B, and consequently, that their elastic forces must be equal in the mixture, and also equal to the elastic force of the mixture.

The addition of two other propositions will not only give the means of comparing the result of the preceding one with experiment, but also give the formulæ which will supply the place of Mr. Dalton's.

Proposition II.—If given volumes V, v, of gases of different elastic forces F, f, be allowed to mix and occupy the volumes which previously contained them, the elastic force of the mixture will be equal to $\frac{VF + vf}{V + v}$.

Let p be the elastic force of the mixture: and since it has been proved that each gas taken separately must be of the same elastic force as the mixture, and the volumes are inversely as the elastic forces, we have

$\frac{1}{f} : \frac{1}{p} :: v : \frac{vf}{p}$ = the volume of the gas whose elastic force was f before mixture; and consequently,

$V + v - \frac{vf}{p} = \frac{p(V + v) - vf}{p}$ = the volume to be occupied by the other gas. Hence

$$\frac{1}{V} : \frac{p}{p(V+v)-vf} :: F : p = \frac{Vrp}{p(V+v)-vf}, \text{ or } \frac{VF + vf}{V + v} = p.$$

Cor. 1.—When the volumes before mixture are equal $\frac{F+f}{2} = p$; or the resulting elastic force is the mean between the elastic forces before mixture.

Cor. 2.—If $F = f$, then $F = p$, or the elastic force, is not changed by mixture.

PROPOSITION III.—If given volumes V, v, of gases or vapours of different elastic forces F, f, be mixed, and the elastic force of the mixture be p, then $\frac{VF + vf}{p}$ = the volume of the mixture.

For, by Prop. 1, the elastic force of each gas is to be equal to the elastic force of the mixture, and therefore

$\frac{1}{F} : \frac{1}{p} :: V : \frac{VF}{p}$ = the volume of the gas whose force before mixture was F; and

$\frac{1}{f} : \frac{1}{p} :: v : \frac{vf}{p}$ = the volume of the gas whose force was f before mixture: hence, the volume of the resulting compound is $\frac{VF + vf}{p}$.

Cor.—When $V = v$, and $F = p$, we have

$\frac{V(p+f)}{p}$ = the volume after mixture.

This condition, viz. that $V = v$, seems to apply with accuracy to the combination of air with the vapour of water, when the air is saturated. Mr. Dalton arrives at the formula $\frac{Vp}{p-f}$ = the volume; and to put the two to the test, let an experiment be made when f is equal to $\frac{2}{3}p$.

By Mr. Dalton's formula, the volume of the mixture of air and vapour would be three times the volume of the dry air.

By my formula the volume of the mixture of air and vapour would be only $1\frac{2}{3}$ of the volume of the dry air.

I did intend to conclude here; but I cannot resist the temptation to ask Mr. Dalton, or M. Gay-Lussac, how in a mixture of one part of dry air of an elastic force of 30 inches of mercury with 2 parts of vapour of the elastic force of only 20 inches, the whole mixture should possess an elastic force of 30 inches? If they can answer this question satisfactorily, we need not altogether despair of a perpetual motion being discovered. But to be once again serious: I shall be very happy to have any error in my train of reasoning or results pointed out, should such be detected by any of your learned contributors.

16, Grove Place, Lisson Grove, May 2, 1826.

L. *On the Equilibrium of the Funicular Curve when the String is extensible. By* H. MOSELEY, *Esq. B.A.**

LET the forces acting on the point (x, y) of the curve in the directions of the axes be X Y.

Let the length of the corresponding branch of the curve be (s) and the tension at its extremity T.

Then since by a property of the funicular polygon all the forces acting on the branch (s) if applied at its extremity would be in equilibrum with the tension (T) at that point, we have, calling μ the mass of an unit of (s), and S its length before distension, the mass of each linear unit being in this case considered unity,

$$\int X\mu ds + T\frac{dx}{ds} = 0 \quad \dots\dots\dots \quad (1)$$

$$\int Y\mu ds + T\frac{dy}{ds} = 0 \quad \dots\dots\dots \quad (2)$$

$$ds - \left(1 + \frac{T}{E}\right) dS = 0 \quad \dots\dots\dots \quad (3)$$

(E being the modulus of extension)

$$dS - \mu ds = 0 \quad \dots\dots\dots\dots \quad (4)$$

From the two first equations, we get

$$\left.\begin{array}{l} y\int X\mu ds - x\int Y\mu ds \\ + T\frac{ydx - xdy}{ds} \end{array}\right\} = 0$$

Now, $\frac{xdy - ydx}{ds}$ = the perpendicular on the tangent $= p$ (suppose), ∴ differentiating S we obtain (observing that $dy \int X\mu ds - dx\int Y\mu ds = 0$)

$$(Xy - Yx)\mu ds - d(Tp) = 0 \quad \dots\dots \quad (\alpha)$$

Again, differentiating the equations (1) and (2) multiplying the former by dx and the latter by dy and adding, we get

$$dT + (Xdx + Ydy)\mu = 0 \dots\dots\dots (\beta)$$

And by equations (3) and (4),

$$\mu = \frac{1}{1 + \frac{T}{E}}$$

$$\frac{(Xy - Yx)ds}{1 + \frac{T}{E}} - d(Tp) = 0$$

$$\frac{(Xdx + Ydy)}{1 + \frac{T}{E}} + dT = 0$$

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* Communicated by the Author.

from the last equation

$$\int(X\,dx + Y\,dy) + T + \frac{1}{2}\,\frac{T^2}{E} = C$$

$$\therefore T^2 + 2TE = CE - 2\int(X\,dx + X\,dy)\,E$$

$$\therefore T = -E \pm \sqrt{E^2 + \{C - 2\int(X\,dx + Y\,dy)\}E}$$

if one extremity of the string be free so that at this extremity $T = 0$, and the integral be taken from this point as a limit, then $C = 0$, and the equilibrium becomes impossible, unless

$$E > 2\int(X\,dx + Y\,dy).$$

In the impossible case, a continual motion will be communicated to the string by the action of the forces upon it.

In the case in which the extremities of the string are joined: if M be the value of $\int(X\,dx + Y\,dy)$ taken through the arc (s), and N its value taken throughout the whole length plus the quantity s, we have, since in the latter case T becomes $-$ T

$$T^2 + 2TE = C - 2ME$$

$$T^2 - 2TE = C - 2NE$$

$$\therefore \quad 2T = (N - M)$$

$$T = \frac{N - M}{2}.$$

To determine the equation to the curve, we have generally, if $\{C - 2\int(X\,dx + Y\,dy)\}\,E = K$.

$$\mu = \frac{E}{E+T} = \left(\frac{E}{E+K}\right)^{\frac{1}{2}}$$

$$T = -E \pm (E^2 + KE)^{\frac{1}{2}}$$

$$\therefore -E \pm (E^2 + KE)^{\frac{1}{2}} + \int\frac{E^{\frac{1}{2}}(X\,dx + Y\,dy)}{(E + K)^{\frac{1}{2}}} = C.$$

Whence the curve may in all cases be determined. Also when the force acts from a centre, we have $Xy - Yx = 0$ $\therefore$ by the equation (α)

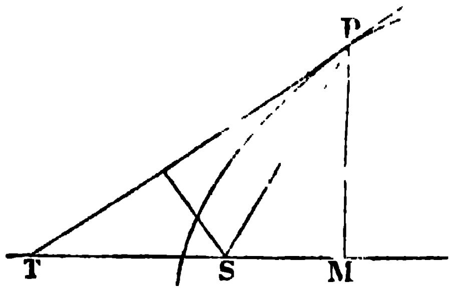
$$d(Tp) = 0$$

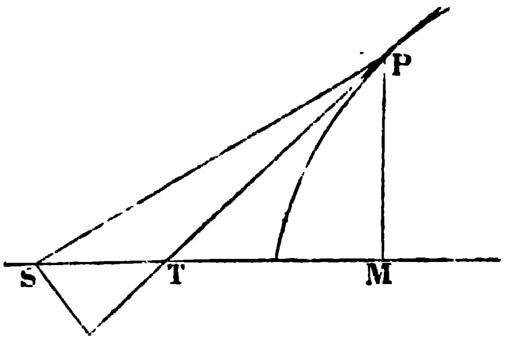
$$\therefore p = \frac{C}{-E \pm (E^2 + XE)^{\frac{1}{2}}}$$

the double value of (p) for a given value of (r), shows two positions of equilibrium; in the case in which p is negative

$$\because \frac{y\,dx - x\,dy}{ds} < 0 \therefore \frac{y}{x} < \frac{dy}{dx}.$$

As





Or the angle PSM < PTX and ∴ the pole S lies without the curve as in fig. 2.

In this case the equilibrium is stable when the force is attractive, and unstable when repulsive.

In the other case the curve may be convex or concave to the pole, or both.

In the case in which the extremities of the string are joined, we have but one position of equilibrium, and

$$p = \frac{2}{M-N}.$$

In this case the form of the curve will be the same as though it were inextensible.

LI. *On the mutual Action of Sulphuric Acid and Naphthaline, and on a new Acid produced.* By M. Faraday, *Esq. F.R.S. Corresponding Member of the Royal Academy of Sciences, &c. &c.*[*]

IN a paper "On new compounds of carbon and hydrogen[†]," lately honoured by the Royal Society with a place in the Philosophical Transactions, I had occasion briefly to notice, the peculiar action exerted on certain of those compounds by sulphuric acid. During my attempts to ascertain more minutely the general nature of this action, I was led to suspect the occasional combination of the hydro-carbonaceous matter with the acid, and even its entrance into the constitution of the salts, which the acid afterwards formed with bases. Although this opinion proved incorrect, relative to the peculiar hydrocarbons forming the subject of that paper, yet it led to experiments upon analogous bodies, and amongst others, upon naphthaline, which terminated in the production of the new acid body and salts now to be described.

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[*] From the Philosophical Transactions for 1825, Part II.

[†] See Philosophical Magazine, vol. lxvi. p. 180.

Some of the results obtained by the use of the oil gas products are very peculiar. If, when completed, I find them sufficiently interesting, I shall think it my duty to place them before the Royal Society, as explicatory of that action of sulphuric acid which was briefly noticed in my last paper.

Most authors who have had occasion to describe naphthaline, have noticed its habitudes with sulphuric acid. Mr. Brande, several years since* stated that naphthaline dissolved in heated sulphuric acid "in considerable abundance, forming a deep violet coloured solution, which bears diluting with water without decomposition. The alkalies produce in this solution a white flaky precipitate, and if diluted the mixture becomes curiously opalescent, in consequence of the separation of numerous small flakes." The precipitate by alkali was probably one of the salts to be hereafter described.

Dr. Kidd observes †, that "it blackens sulphuric acid when boiled with it; the addition of water to the mixture having no other effect than to dilute the colour, neither does any precipitation take place upon saturating the acid with ammonia."

Mr. Chamberlain states ‡, that sulphuric acid probably decomposes naphthaline, for that it holds but a very small quantity in solution. The true interpretation of these facts and statements will be readily deduced from the following experimental details.

1. *Production and Properties of the new Acid formed from Sulphuric Acid and Naphthaline.*

Naphthaline, which had been almost entirely freed from naphtha by repeated sublimation and pressure, was pulverized; about one part with three or four parts by weight of cold sulphuric acid were put into a bottle, well shaken, and left for 36 hours. The mixture then contained a tenacious deep red fluid, and a crystalline solid; it had no odour of sulphurous acid. Water being added, all the liquid and part of the solid was dissolved; a few fragments of naphthaline were left, but the greater part was retained in solution. The diluted fluid being filtered was of a light brown tint, transparent, and of an acid and bitter taste.

For the purpose of combining as much naphthaline as possible with the sulphuric acid, 700 grains, with 520 grains of oil of vitriol were warmed in a Florence flask until entirely fluid, and were well shaken for about 30 minutes. The mixture was red; and the flask being covered up and left to cool, was found after some hours to contain, at the bottom, a little brownish fluid, strongly acid, the rest of the contents having solidified into a highly crystalline mass. The cake was removed, and its lower surface having been cleaned, it was put into another Florence flask with 300 grains more of naphthaline, the whole melted and well shaken together, by which an uniform mixture was obtained; but opaque and dingy in colour. It was now poured into glass tubes, in which it could be retained and examined without contact of air. In these the substance was observed to divide into two portions, which could easily be distinguished from each other, whilst both were retained in the fluid state. The heavier portion was in the largest quantity; it was of a deep red colour, opaque in tubes half an inch in diameter, but in small tubes could be seen through by a candle, or sun light, and appeared perfectly clear. The upper portion was also of a deep red colour, but clear, and far more transparent than the lower: the line of separation very defined. On cooling the tubes, the lighter substance first solidified, and after some time the heavier substance also became solid. In this state, whilst in the tube, they could with great difficulty be distinguished from each other.

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* Quarterly Journal of Science, viii. p. 289. 1819.

† Philosophical Transactions, 1821, p. 216.

‡ Annals of Philosophy, N.S. vi. p. 136. 1823.

These two substances were separated, and being put into tubes, were further purified by being left in a state of repose at temperatures above their fusing points, so as to allow of separation; and when cold, the lower part of the lighter substance, and the upper, as well as the lower part of the heavier substance, were set aside for further purification.

The *heavier substance* was a red crystalline solid, soft to the nail like a mixture of wax and oil. Its specific gravity was from 1·3 to 1·4, varying in different specimens; its taste sour, bitter, and somewhat metallic. When heated in a tube, it fused, forming as before a clear but deep red fluid. Further heat decomposed it, naphthaline, sulphurous acid, charcoal, &c. being produced. When heated in the air it burnt with much flame. Exposed to air it attracted moisture rapidly, became brown and damp upon the surface, and developed a coat of naphthaline. It dissolved entirely in alcohol, forming a brown solution. When rubbed in water a portion of naphthaline separated, amounting to 27 per cent. and a brown acid solution was obtained. This was found by experiments to contain a peculiar acid mixed with a little free sulphuric acid, and it may conveniently be called *the impure acid.*

The *lighter substance* was much harder than the former, and more distinctly crystalline. It was of a dull red colour, easily broken down in a mortar, the powder being nearly white, and adhesive like naphthaline. It was highly sapid, being acid, bitter, and astringent. When heated in a tube it melted, forming a clear red fluid, from which by a continued heat much colourless naphthaline sublimed, and a black acid substance was left, which at a high temperature gave sulphurous acid and charcoal. When heated in the air it took fire and burnt like naphthaline. Being rubbed in a mortar with water, a very large portion of it proved to be insoluble; this was naphthaline; and on filtration the solution contained the peculiar acid found to exist in the *heavier substance*, contaminated with very little sulphuric acid. More minute examination proved that this *lighter substance* in its fluid state was a solution of a small quantity of the dry peculiar acid in naphthaline; and that the *heavier substance* was an union of the peculiar acid in large quantity with water, free sulphuric acid, and naphthaline.

It was easy by diminishing the proportion of naphthaline to make the whole of it soluble, so that when water was added to the first result of the experiment, nothing separated; and the solution was found to contain sulphuric acid with the peculiar acid. But reversing the proportions, no excess of naphthaline was competent, at least in several hours, to cause the entire disappearance of the sulphuric acid. When the experiment was carefully made with pure naphthaline, and either at common, or slightly elevated temperatures, no sulphurous acid appeared to be formed, and the action seemed to consist in a simple union of the concentrated acid and the hydro-carbon.

Hence it appears, that when concentrated sulphuric acid and naphthaline are brought into contact at common, or moderately elevated temperatures, a peculiar compound of sulphuric acid with the elements of the naphthaline is produced, which possesses acid properties; and as this exists in large quantity in the heavier of the bodies above described, that product may conveniently be called the *impure solid acid.* The experiments made with it, and the mode of obtaining the pure acid from it, are now to be described.

Upon applying heat and agitation to a mixture of one volume of water and five volumes of impure solid acid, the water was taken up to the exclusion of nearly the whole of the free naphthaline present; the latter separating in a colourless state from the red hydrated acid beneath it. As the temperature of the acid diminished, crystallization in tufts commenced here and there, and ultimately the whole became a brownish yellow solid. A sufficient addition of water dissolved nearly the whole of this hydrated acid, a few flakes only of naphthaline separating.

A portion

A portion of the impure acid in solution was evaporated at a moderate temperature; when concentrated, it gradually assumed a light brown tint. In this state it became solid on cooling, of the hardness of cheese, and was very deliquescent. By further heat it melted, then fumed, charred, &c. and gave evidence of the abundant presence of carbonaceous matter.

Some of the impure acid in solution was neutralized by potash, during which no naphthaline or other substance separated. The solution being concentrated until ready to yield a film on its surface, was set aside whilst hot to crystallize: after some hours the solution was filled with minute silky crystals, in tufts, which gave the whole, when stirred, not the appearance of mixed solid salt and liquid, but that of a very strong solution of soap. The agitation also caused the sudden solidification of so much more salt, that the whole became solid, and felt like a piece of soft soap. The salt when dried had no resemblance to sulphate of potash. When heated in the air, it burnt with a dense flame, leaving common sulphate of potash, mixed with some sulphuret of potassium, resulting from the action of the carbon, &c. upon the salt.

Some of the dry salt was digested in alcohol to separate common sulphate of potash. The solution being filtered and evaporated, gave a white salt soluble in water and alcohol, crystalline, neutral, burning in the air with much flame, and leaving sulphate of potash. It was not precipitated by nitrate of lead, muriate of baryta, or nitrate of silver.

It was now evident that an acid had been formed peculiar in its nature and composition, and producing with bases peculiar salts. In consequence of the solubility of its barytic salt, the following process for the preparation of the pure acid was adopted:

A specimen of native carbonate of baryta was selected, and its purity ascertained. It was then pulverized, and rubbed in successive portions with a quantity of the impure acid in solution, until the latter was perfectly neutralized, during which the slight colour of the acid was entirely removed. The solution was found to contain the peculiar barytic salt. Water added to the solid matter dissolved out more of the salt; and ultimately only carbonate and sulphate of baryta, mixed with a little of another barytic salt, remained. The latter salt being much less soluble in water than the former, was not removed so readily by lixiviation, and was generally found to be almost entirely taken up by the last portions of water applied with heat.

The barytic salt in solution was now very carefully decomposed, by successive additions of sulphuric acid, until all the baryta was separated, no excess of sulphuric acid being permitted. Being filtered, a pure aqueous solution of the peculiar acid was obtained. It powerfully reddened litmus paper, and had a bitter acid taste. Being evaporated to a certain degree, a portion of it was subjected to the continued action of heat; when very concentrated it began to assume a brown colour, and on cooling became thick, and ultimately solid, and was very deliquescent. By renewed heat it melted, then began to fume, charred, but did not flame; and ultimately gave sulphuric and sulphurous acid vapours, and left charcoal.

Another portion of the unchanged strong acid solution was placed over sulphuric acid in an exhausted receiver. In some hours it had by concentration become a soft white solid, apparently dry; and after a longer period was hard and brittle. In this state it was deliquescent in the air, but in close vessels underwent no change in several months. Its taste was bitter, acid, and accompanied by an after metallic flavour, like that of cupreous salts. When heated in a tube at temperatures below 212°, it melted without any other change; and on being allowed to cool, crystallized from centres, the whole ultimately becoming solid. When more highly heated, water at first passed off, and the acid assumed a slight red tint; but no sulphurous acid was as yet produced, nor any charring occasioned; and a portion being dissolved and tested by muriate of baryta, gave but a very minute trace of free sulphuric acid. In this state it was probably anhydrous. Further heat caused a little naphthaline to rise, the red colour became deep brown, and then a sudden action commenced at the bottom of the tube, which spread over the whole, and the acid became black and opaque. Continuing the heat, naphthaline, sulphurous acid, and charcoal were evolved; but even after some time the residuum examined by water and carbonate of baryta, was found to contain a portion of the peculiar acid undecomposed, unless the temperature had been raised to redness.

These facts establish the peculiarity of this acid, and distinguish it from all others. In its solid state it is generally a hydrate containing much combustible matter. It is readily soluble in water and alcohol, and its solutions form neutral salts with bases, all of which are soluble in water, most of them in alcohol, and all combustible, leaving sulphates or sulphurets according to circumstances. It dissolves in naphthaline, oil of turpentine, and olive oil, in greater or smaller quantities, according as it contains less or more water. As a hydrate, when it is almost insoluble in naphthaline, it resembles the *heavier substance*, obtained, as before described, by the action of sulphuric acid on naphthaline, and which is the solid hydrated

acid,

acid, containing a little naphthaline, and some free sulphuric acid; whilst the *lighter substance* is a solution of the dry acid in naphthaline; the water present in the oil of vitriol originally used being sufficient to cause a separation of a part, but not of the whole.

[To be continued.]

LII. *Hydrographical Notices:—Remarks on the Method of investigating the Direction and Force of the Currents of the Ocean; Presence of the Water of the Gulf-Stream on the Coasts of Europe in January 1822; Summary of the Currents experienced by His Majesty's Ship Pheasant, in a Voyage from Sierra Leone to Bahia, and thence to New York; Stream of the River Amazons crossed, three hundred Miles from the Mouth of the River. By Capt.* EDWARD SABINE, *R.A. F.R. & L.S. &c.**

PREVIOUSLY to my leaving England in 1821, I had had the great advantage of much conversation with Major Rennell, on the subject of the currents in the northern and southern Atlantic Oceans, and of having my attention directed by him to those points in particular, concerning their velocity, limits, and temperature, on which further inquiries might conduce to the advancement of hydrographical knowledge.

The method of ascertaining the existence, direction and velocity of a current, where land is not in sight, and a ship cannot be rendered stationary by anchorage, is to compare her position at intervals of sufficient length (generally of 24 hours) by observation and by reckoning. By the former is learnt her real change of geographical position in the interval; by the latter, the course and distance that she has gone through the water; should the position by the reckoning not agree with the position by the observation, the difference (presuming both to be correct) is the indication and measure of current.

To determine a ship's position from day to day by observation, or rather, her relative position on one day to the preceding, has become, since the introduction of chronometers, a matter of very simple accomplishment, and capable of much precision. It is far otherwise with the reckoning, however, when more is sought by it than such a rough approximation as may serve the ordinary purposes of navigation: it must, in fact, require the most assiduous and unremitting attention, as well as considerable nautical experience and judgement, to estimate correctly the continually varying effects of the winds and sea, on a body that is also continually varying the measure of her exposure to their influence. It may be in the power of an individual in a vessel, to obtain, by his own exertions alone, that portion of the materials towards the evidence of currents, which depends on her real change of position; but the completion of the evidence by a sufficiently correct reckoning must be the result of an interest participated in by all the executive officers of a ship; or by the establishment of such habits of accuracy, under the authority of her commander, as are not of usual practice, because they are not necessary for the general purposes of navigation; the employment of chronometers, by which the position of a ship is ascertained and a fresh departure taken on every day that the sun shines, has superseded the necessity of that vigilant and scrupulous regard, which the older navigators paid to all the details of the reckoning, on which alone they had to depend; and has tended to substitute general habits of loose and vague estimation, for the considerate and well-practised judgement with which allowances were formerly made for the incidental circumstances of steerage, leeway, making and shortening sail, &c. &c., on a due attention to which the accuracy of a reckoning so materially depends.

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* From Captain Sabine's newly-published Account of his Experiments to determine the Figure of the Earth.

In ships of war especially, the reckoning is further embarrassed by a difficulty less obvious, but not less generally operative, by which, if not properly provided against, the knowledge of the true course which the ship has made is necessarily rendered very uncertain: it arises from the usual practice of directing the course by the binnacle compasses, which are two in number for the convenience of the helmsmen, and being placed one on the larboard and the other on the starboard side of the midship, with a space between them of greater or less extent according to the size of the vessel, can scarcely fail, and are, in fact, generally influenced differently by the ship's iron; and being subject to different *systems* of attraction, the compasses not only disagree, but their disagreement varies according to the direction of ship's head, the amount of the dip of the needle, and the force of terrestrial magnetism. It is customary always to steer by the weather compass; and thus each is liable to become in its turn the directing compass for periods of more or less duration, and the corrections of the courses for the disturbing influence of the ship's iron, becomes so various and complicated, as to render the deduction of a correct reckoning practically unattainable. For example, the binnacle compasses of the Iphigenia, on her passage from England

land to Madeira, were observed to differ from each other half a point in one direction when on south-westerly courses, and less than half a point in the opposite direction when on easterly courses, the indications of the compasses having crossed each other, and agreed at some intermediate point; it was requisite, therefore, that the correction to be allowed on every course by each of the two compasses should be ascertained, and that the compass by which each course was directed should be specially recorded, in order that the true course should be known.

The most obvious mode of preventing so much inconvenience and trouble, as well as the more correct practice, is to direct and note the ship's course by one compass only, stationed permanently in some convenient situation, without reference to the helmsmen, and to use the binnacle compasses solely to steer by, on the point which may be noticed at the time to agree with the magnetic course of the standard compass; and by employing an azimuth compass for the latter purpose, the advantage is gained of enabling the variation to be observed directly with the compass by which the course is governed, and thus of avoiding intermediate comparisons, in which time is occupied, and errors frequently introduced. This arrangement of a standard compass was adopted by Captain Clavering in the Pheasant, and subsequently in the Griper, and was found to answer its purpose perfectly, and to be attended with no practical inconvenience whatsoever.

Although from the courses above noticed, no satisfactory investigation of the direction or velocity of currents could be made in the Iphigenia, in her passage from England to the coast of Africa, a remarkable and very interesting evidence was obtained by observations on the temperature of the sea, of the accidental presence in that year of the water of the Gulf-stream, in longitudes much to the eastward of its ordinary extension. The Iphigenia sailed from Plymouth on the 4th of January, after an almost continuous succession of very heavy westerly and south-westerly gales, by which she had been repeatedly driven back and detained in the ports of the Channel. The following memorandum exhibits her position at noon on each day of her subsequent voyage from Plymouth to Madeira, and from thence to Cape Verd Islands, the temperature of the air in the shade and to windward, and that of the surface of the sea; it also exhibits in comparison, the ordinary temperature of the ocean at that season, in the respective parallels, which Major Rennell has been so kind as to permit me to insert on his authority, as an approximation founded on his extensive inquiries; the last column shows the excess or defect in the temperature observed in the Iphigenia's passage.

Date.		Latit. N.	Longit. W.	Air.	Surface Water.		Excess or Defect.
					Observed.	Usual.	
	1822.	° ′	° ′	°	°	°	
Plymouth to Madeira.	Jan. 5	47 30	7 30	47	49	50	−1
	6	44 20	9 30	52·5	55·7	52·5	+3·2
	7	41 22	11 37	54	58·2	54	+4·2
	8	38 54	13 20	54·2	61·7	55·7	+6
	9	no observ.		56	63	58	+5
	10	33 40	15 30	60·7	64	60	+4
Madeira to the C. Verds.	19	26 00	17 50	66	65·5	67	−1·5
	20	24 30	18 50	68	67	68·4	−1·4
	21	23 06	20 00	69	69	69·5	−0·5
	22	21 02	21 27	69·5	69·5	71·2	−1·7
	23	19 20	23 00	70·6	70·2	71·6	−1·4

It is seen by the preceding memorandum, that in the passage from Plymouth to Madeira, the Iphigenia found the temperature of the sea, between the parallels of $44\frac{1}{3}^\circ$ and $33\frac{2}{3}^\circ$, several degrees warmer than its usual temperature in the same season; namely, 3°·2 in $44\frac{1}{3}^\circ$, increasing to 6° in 39°, and again diminishing to 4° in $33\frac{2}{3}^\circ$; whilst at the same period, the general temperature of the ocean in the adjoining parallels, both to the northward and to the southward, even as far as the Cape Verd Islands in $19\frac{2}{3}^\circ$, was colder by a degree and upwards than the usual average. The evidence of many careful observers at different seasons and in different years, whose observations have been collected and compared by Major Rennell, has satisfactorily shown, that the water of the Gulf-stream, distinguished by the high temperature which it brings from its origin in the Gulf of Mexico, is not usually found to extend to the eastward of the Azores. Vessels navigating the ocean between the Azores and the continent of Europe, find at all seasons a temperature progressively increasing as they approach the sun; the absolute amount varies according to the season, the maximum in summer being about 14 degrees warmer than the maximum in winter; but the progression in respect to latitude is regular, and is nearly the same in winter as in summer, being an increase of 3° of Fahrenheit for every 5° of latitude. It is further observed, that the ordinary condition of the temperature, in the part of the ocean under notice, is little subject to disturbance, and that in any particular parallel and season, the limits of variation in different years are usually very small: after westerly winds of much strength or continuance, the sea in all the parallels is rather colder than the average temperature, on account of the increased velocity communicated to the general set of the waters of the north-eastern Atlantic towards the southward. To the heavy westerly gales which had prevailed almost without intermission in the last fortnight in November, and during the whole of December, may therefore be attributed the colder temperatures observed in the latitude of 47½°, and in those between 26° and 19⅓°.

If doubt could exist in regard to the higher temperatures between 44⅓° and 33⅔°, being a consequence of the extension in that year of the Gulf-stream in the direction of its general course, it might be removed by a circumstance well deserving of notice; namely, that the greatest excess above the natural temperature of the ocean was found in or about the latitude of 39°, being the parallel where the middle of the stream, indicated by the warmest water, would arrive, by continuing to flow to the eastward of the Azores, in the prolongation of the great circle in which it is known to reach the mid Atlantic.

One previous and similar instance is on record, in which the water of the Gulf-stream was traced by its temperature quite across the Atlantic to the coasts of Europe; this was by Dr. Franklin, in a passage from the United States to France, in November 1776 *. The latter part of his voyage, *i.e.* from the meridian of 35° to the Bay of Biscay, was performed with little deviation in the latitude of 45°; in this run, exceeding 1200 miles, in a parallel of which the usual temperature, towards the close of November, is about 55½°, he found 63° in the longitude of 35° W., diminishing to 60° in the Bay of Biscay; and 61° in 10° west longitude, near the same spot where the Iphigenia found 55°·7 on the 6th of January, being about five weeks later in the season. At this spot then, where the Iphigenia crossed Dr. Franklin's track, the temperature in November 1776 was 5½°, and in January 1822, 3°·2 above the ordinary temperature of the season.

There can be little hesitation in attributing the unusual extension of the stream in particular years to its greater initial velocity, occasioned by a more than ordinary difference in the levels of the Gulf of Mexico and of the Atlantic: it has been computed by Major Rennell, from the known velocity of the stream at various points of its course, that in the summer months, when its rapidity is greatest, the water requires about eleven weeks to run from the outlet of the Gulf of Mexico to the Azores, being about 3000 geographical miles; and he has further supposed, in the case of the water, of which the temperature was examined by Dr. Franklin, that perhaps not less than three months were occupied in addition by its passage to the coasts of Europe, being altogether a course exceeding 4000 geographical miles. On this supposition, the water of the latter end of November 1776, may have quitted the Gulf of Mexico, with a temperature of 83° in June; and that of January 1822, towards the end of July, with nearly the same temperature. The summer months, particularly July and August, are those of the greatest initial velocity of the stream, because it is the period when the level of the Caribbean sea and Gulf of Mexico is most deranged.

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* Franklin's Works, 8vo, London 1806, vol. ii. pp. 200, 201.

It is not difficult to imagine, that the space between the Azores and the coasts of the old continent, being traversed by the stream, slowly as it must be, at a much colder season in the instance observed by the Iphigenia than in that by Dr. Franklin, its temperature may have been cooled thereby to a nearer approximation to the natural temperature of the ocean in the former than in the latter case; and that the difference between the excess of 5°·5 in November, and of 3°·2 in January, may be thus accounted for.

If the explanation of the apparently very unusual facts observed by Dr. Franklin in 1776, and by the Iphigenia in 1822, be correct, how highly curious is the connexion thus traced between a more than ordinary strength of the winds within the tropics in the summer, occasioning the derangement of the level of the Mexican and Caribbean seas, and the high temperature of the sea between the British Channel and Madeira, in the following winter.

Nor is the probable meteorological influence undeserving of attention, of so considerable an increase in the temperature of the surface-water over an extent of ocean exceeding 600 miles in latitude and 1000 in longitude, situated so importantly in relation to the western parts of Europe. It is at least a remarkable coincidence; that in November and December 1821, and in January 1822, the state of the weather was so unusual in the southern parts of Great Britain and in France, as to have excited general observation; in the meteorological journals of the period it is characterized "as most extraordinarily hot, damp, stormy, and oppressive:" it is stated "that an unusual quantity of rain fell both in November and December, but particularly in the latter*;" that, "the gales from the west and south-west were almost without intermission," and that in December, the mercury in the barometer was lower than it had been known for 35 years before*.

On leaving the Cape Verd Islands, the Iphigenia proceeded to make the continent of Africa at Cape Verd. The distance between the Cape and the Islands is about 400 miles, both being in the same parallel of latitude. This passage afforded an interesting opportunity of observing on the approach to land, the influence of its vicinity on the temperature of the sea. The general temperature of the surface in that parallel and at that season may be considered 71°·7, the observations made at sunrise, noon, and sunset, in the first 350 miles of the passage, varying from 71° to 72°·4: but at sunrise on the 31st of January, being then at the distance of 26 miles west of Cape Verd, with no land as yet in sight, the surface-water had lowered to 69°·6. On approaching nearer it progressively diminished, until at one mile from the shore, it had fallen as low as 64 degrees, and continued from 64 to 65 degrees, between Cape Manoel and Goree. Cape Verd is situated nearly at equal distances, exceeding 70 miles, from the mouths of the Senegal and Gambia, the one being to the north and the other to the south. It is probable that the water of both these rivers is always colder at their entrance into the sea, than the ocean temperature of the parallel; that of the Gambia certainly was so at that season, but it was not so cold as the sea in the vicinity of Cape Verd, as on approaching the entrance of the Gambia, the temperature of the surface rose to 67°·5, and varied in the river itself at different hours from 66° to 67°·5; and at the depth of 36 feet, being within six feet of the bottom, a self-registering thermometer indicated at high water less than a degree colder than the surface. The coast in the neighbourhood of Cape Verd is every where low and sandy, and is covered with trees to the water's edge. Such, indeed, is the general character of the shores of western Africa, with the exception of Cape Sierra Leone; but at no other part of the coast was the diminution of the temperature of the water, on approaching the land, so great, as in the instance which has been mentioned. Between the Gambia and Sierra Leone are a succession of rivers, originating in land of less elevation than the Senegal and Gambia, and much exceeding them in the temperature of the waters which they convey into the ocean; in the mid-channel of the Rio Grande, at a few miles from its mouth, the surface was never less than 74°, and occasionally as high as 77°·5, and at the depth of 30 or 40 feet was less than a degree colder than the surface. At the entrance of the River Noonez the surface-water was 77°·5, and at that of the Rokelle 80°. To the south of the Rokelle, and from thence to the extremity of the Gulf of Guinea, the coast is swept by a current of considerable rapidity, which renders the cooling effect of the land less apparent; but in the bays of the coast, where the current sweeps from point to point, and leaves still water in the inside, a difference is commonly found amounting to three and four degrees*.

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* The following description of this very remarkable winter is extracted from Mr. Daniell's Essay on the Climate of London (Meteorological Essays, London 1823, pages 297 and 298), and becomes highly curious when viewed in connexion with the unusual temperature of the ocean in the direction from which the principal winds proceeded.

"November 1821, differed from the mean, and from both the preceding years, in a very extraordinary way. The average temperature was 5° above the usual amount, and although its dryness was in excess," [the relative dryness, in consequence of the increased temperature] "the quantity of rain exceeded the mean quantity by one half. The barometer on the whole was not below the mean. All the low lands were flooded, and the sowing of wheat very much interrupted by the wet.

"In December, the quantity of rain was very nearly double its usual amount. The barometer averaged considerably below the mean, and descended lower than had been known for 35 years. Its range was from 30·27 inches to 28·12 inches. The temperature was still high for the season, and the weather continued, as in the last month, in an uninterrupted course of wind and rain; the former often approaching to an hurricane, and the latter inundating all the low grounds. The water-sodden state of the soil, in many parts, prevented wheat sowing, or fallowing the land at the regular season. The mild temperature pushed forward all the early sown wheats to an height and luxuriance scarcely ever before witnessed. The grass, and every green production, increased in an equal proportion.

January 1822. This most extraordinary season still continued above the mean temperature, but the rain, as if exhausted in the preceding month, fell much below the usual quantity in this. There was not one day on which the frost lasted during the twenty-four hours.

"Serious apprehensions were entertained lest the wheats, drawn up as they had been by warm and moist weather, without the slightest check from frost, should be exhausted by excessive vegetation, and ultimately be more productive in straw than corn.

"The month of February, still five degrees above the mean temperature, ended a winter which has never been paralleled."

It would not be difficult to trace in detail, each of the effects described in the preceding extract, to the cause which has been thus placed in connexion with them.

[To be continued.]

LIII. *On*

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* The passage from the Cape Verd Islands to Cape Verd and the Gambia, afforded a not less interesting opportunity of observing the difference in the hygrometrical state of the atmosphere at sea, and in the vicinity of the continent, in the region of the trade winds. We had entered the N.E. trade in the latitude of 24° North, nine degrees to the northward of the Cape Verd Islands, and did not lose it until the afternoon of the day on which we quitted the Gambia, the strength declining on the approach to the continent, but the direction continuing unchanged. On the 28th, 29th, and 30th of January, in navigating the first 350 miles of the passage from the islands to the continent, the air in the shade and to windward varied at different hours of the day from 70°·2 to 71°·2, and the dew-point from 63° to

LIII. *On the Properties of a Line of shortest Distance traced on the Surface of an oblate Spheroid.* *By* J. Ivory, *Esq. M.A. F.R.S.**

[Concluded from p. 249.]

In continuing the subject of my last communication, I shall now examine particularly the case of a geodetical line directed at right angles to the meridian. For this purpose I resume the formula before found, viz.

$$\sin u = \sin l \cos z,$$

$$d s = \frac{(1 + e^2) \sqrt{1 + e^2 \cos^2 l} \, . \, d z}{(1 + e^2 - e^2 \sin^2 l \cos^2 z)^{\frac{3}{2}}},$$

l being the latitude at the commencement of the line, and u the latitude at its termination. We rejected this formula, because the arc z cannot be safely determined by means of the latitudes. But this objection will be of no force if the same arc can be ascertained with sufficient exactness either by the difference of longitude, or the change in azimuth. In reality the formula is extremely proper for finding s: for so long as z is not very considerable the denominator varies slowly, and z is almost proportional to s. We may likewise illustrate the

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to 64°·5. At sunrise on the 31st, when at 26 miles west of Cape Verd, the dew-point was 61°·5, and lowered to 57°·5 on nearing the land, the temperature of the air not being sensibly affected. Off the entrance of the Gambia, on the 1st of February, and in the river on the 2d and 3rd and 4th, the dew-point was never higher than 51°, and occasionally as low as 48°·5, the air over the water and in the shade being generally during the day from 69° to 70°. When about to quit the Gambia on the morning of the 5th of February, we experienced, although in a very slight degree, the peculiar wind called the Harmattan, of which the season was nearly over: its direction was one or two points to the north of the trade wind, or about N.N.E.; the air during its influence fell to 66°·5, and the dew-point to 37°·5; affording a reasonable inference, that in a genuine Harmattan, and before it reaches the sea, the constituent temperature of the vapour may be at least as low as 32°. In the progress to Cape Roxo, on the afternoon of the same day, we lost the Harmattan, and with it the continuance of the trade wind. The sea breeze which followed, raised the temperature of the air to 70°, and of the dew-point to 61°·5.

It appears, therefore, that when the north-east wind first comes off the continent of Africa it contains only 53 parts in 100 of the moisture which would be required for repletion at the existing temperature; that in blowing over the sea its proportion of moisture rapidly augments, until at fifty miles from the land, it has acquired 80 parts in 100; which proportion is not subsequently increased by its passage over 350 additional miles of ocean. In the Harmattan the air contained only 38 parts in 100 of the proportion of moisture required for its repletion.

* Communicated by the Author.

same thing by attending to what z represents on the surface of the sphere. Let the arc i' be determined by this equation; viz.

$$\tan i = \frac{\tan i'}{\sqrt{1+e^2}};$$

and draw a great circle having the inclination i' to the equator, and intersecting it in the same diameter with the former oblique circle. Now let any meridian meet the two circles, and let ψ and u be the arcs of the meridian between the equator, and the respective circles; then we shall have this equation, viz.

$$\tan \psi = \frac{\tan u}{\sqrt{1+e^2}};$$

whence it follows, that if ψ be the latitude of a parallel to the equator on the surface of the sphere, u will be the latitude of the same parallel on the surface of the spheroid. Hence it will readily appear that z is the arc of the latter great circle intercepted between the two meridians that pass through the extremities of the arc s' of the former circle; and, on account of the proximity of the two circles, it is never much different from s' or s. When the meridian is nearly perpendicular to the circles, it is also evident, as has already been observed, that a small error in the latitude will occasion a great variation in z.

Having expanded the foregoing formula and integrated as usual (using Hirsch's tables of fluents, or the tables of any such plodding collector, if need be), we shall get, by neglecting the powers of e^2 above the square,

$$\frac{s}{\mathrm{P}} = z \left\{ 1 + \frac{e^2 \sin^2 l}{4} - \frac{e^4}{64} (16 \sin^2 l - 13 \sin^4 l) \right\}$$
$$+ \sin 2z \left\{ \frac{3e^2}{8} \sin^2 l - \frac{3e^4}{32} (4 \sin^2 l - 3 \sin^4 l) \right\}$$
$$+ \frac{15 e^4 \sin^4 l}{256} \times \sin 4z:$$

and, in this formula, we have only to substitute the value of z in terms of the difference of longitude, or of the change in azimuth.

Let us first compare z with the difference of longitude. The second formula (A) gives us

$$d\phi = \phi' \times \frac{\sqrt{1+e^2 \sin^2 \psi}}{\sqrt{1+e^2}} = \frac{d\phi'}{\sqrt{1+e^2 \cos^2 u}}.$$

Observing that here $\sin \mu = 1$, $\sin i = \sin \lambda$, the formulæ (B) give us

$$-d\phi' = \frac{\cos \lambda \, d\psi}{\cos \psi \sqrt{\sin^2 \lambda - \sin^2 \psi}}.$$

Now we shall find

$$\sqrt{\sin^2\lambda - \sin^2\psi} = \frac{\sqrt{1+e^2}\,.\,\sqrt{\sin^2 l - \sin^2 u}}{\sqrt{1+e^2\cos^2 l\,.\,1+e^2\cos^2 u}},$$

$$\frac{d\psi}{\cos\psi} = \frac{d}{\cos u\,\sqrt{1+e^2\cos^2 u}};$$

and hence, because $\cos l = \frac{\cos\lambda\,\sqrt{1+e^2\cos^2 l}}{\sqrt{1+e^2}}$,

we get, $$-\,d\varphi' = \frac{\cos l\,du}{\cos u\,\sqrt{\sin^2 l - \sin^2 u}},$$

$$d\varphi' = \frac{\cos l\,dz}{\cos^2 l + \sin^2 l\sin^2 z}.$$

We have therefore,

$$d\varphi = \frac{\cos l\,dz}{\cos^2 l + \sin^2 l\sin^2 z} \times \frac{1}{\sqrt{1+e^2(\cos^2 l + \sin^2 l\sin^2 z)}}.$$

If this expression of $d\varphi$ be expanded and integrated, we shall find,

$$\varphi = \text{arc tan.}\left(\frac{\tan z}{\cos l}\right) - z \times \cos l\left\{\frac{e^2}{2} - \frac{3e^4}{8}\cos^2 l\right\}.$$

In this value I have rejected $\frac{3e^4\cos l\sin^2 l}{8}\int dz\sin^2 z$, which is altogether insensible even supposing z equal to 10° or 12°. Next put

$$\tan\varphi\cos l = \tan x,$$

$$\alpha = \tfrac{1}{2}e^2 - \frac{3e^4}{8}\cos^2 l,$$

and the last equation will become

$$\text{arc tan}\left(\frac{\tan x}{\cos l}\right) = \text{arc tan}\left(\frac{\tan z}{\cos l}\right) - \alpha\cos l \times z.$$

We must now find z in terms of x, and, as this operation requires only the ordinary rules of analysis, I shall suppress the detail of the calculation. Neglecting quantities of the order already indicated, I have found,

$$z = x\left\{1 + \frac{e^2}{2}(\cos^2 l + \sin^2 l\sin^2 x) - \frac{e^4}{8}\cos^4 l\right\}, \quad \text{(D)}$$

which formula may likewise be written thus,

$$z = x\sqrt{1+e^2\cos^2 l} + \frac{e^2}{2}\sin^2 l\sin^3 x \;.\;.\;.\;. \quad \text{(D')}$$

This value of z must now be substituted in the formula for s; and, in doing this, it will be sufficient to make

$$\sin 2z = \sin 2x\left(1 + \frac{e^2}{2}\cos^2 l\right) = \sin 2x + e^2\cos^2 l \times x.$$

Thus we get, $$\tan\varphi\cos l = \tan x,$$

$$\frac{s}{\mathrm{P}} = x \left\{ 1 + \frac{e^2}{2}\left(1 - \frac{\sin^2 l}{2} + \sin^2 l \sin^2 x\right) \right.$$

$$\left. - e^4\left(\frac{1}{8} - \frac{\sin^2 l}{2} + \frac{27 \sin^4 l}{64}\right) \right\}$$

$$+ \sin 2x \left\{ \frac{3e^2}{8} \sin^2 l - \frac{3c^4}{32} (4 \sin^2 l - 3 \sin^4 l) \right\}$$

$$+ \frac{15 c^4 \sin^4 l}{256} \times \sin 4x.$$

In this formula the only unknown quantities are e^2 and P, all the coefficients being known, provided the initial latitude and the difference of longitude have been determined by observation.

If the length measured extend to an amplitude of only 2° or 3°, we may make $\cos z = 1$ in the denominator of the differential equation, and then,

$$s = \frac{(1 + e^2) z}{1 + e^2 \cos^2 l}.$$

In this case we likewise obtain from the formula (D′),

$$z = x \sqrt{1 + e^2 \cos^2 l};$$

and hence,
$$s = \frac{(1 + e^2) x}{\sqrt{1 + e^2 \cos^2 l}}.$$

The quantity into which x is here multiplied is the radius of curvature of the geodetical line; or, it is the normal to the surface of the spheroid terminating in the axis of revolution.

Let us next compare z with the change of azimuth. From the fundamental equation (a) we get,

$$\sin \mu' = \frac{\cos \lambda}{\cos \psi} = \frac{\cos l}{\cos u} \cdot \frac{\sqrt{1 + e^2 \cos^2 u}}{\sqrt{1 + e^2 \cos^2 l}}.$$

Put $w = 90^\circ - \mu'$; then,

$$\sin w = \frac{\sqrt{\sin^2 l - \sin^2 u}}{\cos u \sqrt{1 + e^2 \cos^2 l}}.$$

Consequently,

$$\sin w = \frac{\sin l \sin z}{\sqrt{\cos^2 l + \sin^2 l \sin^2 z}} \times \frac{1}{\sqrt{1 + e^2 \cos^2}} l;$$

$$\sin z = \frac{\tan w}{\tan l} \times \frac{\sqrt{1 + e^2 \cos^2 l}}{\sqrt{1 - e^2 \cos^2 l \tan^2 w}}.$$

It appears therefore that z is rigorously determined by the change in azimuth. But it will be better to have recourse to approximation. Assume

$$\frac{\tan w}{\tan l} = \sin y;$$

then
$$\sin z = \sin y \cdot \frac{\sqrt{1 + e^2 \cos^2 l}}{\sqrt{1 - e^2 \sin^2 l \sin^2 y}};$$

or
$$\sin z = \sin y \sqrt{1 + e^2 \cos^2 l} + \frac{e^2}{2} \sin^2 l \sin^3 y.$$

And, by passing from the expression of the sine to that of the arc itself, I have found,

$$z = y\sqrt{1 + e^2\cos^2 l} + \frac{e^2(1 + 2\sin^2 l)}{6}\sin^3 y. \qquad \text{(E)}$$

If we compare this expression with the formula (D′) we shall readily deduce

$$x = y + \frac{e^2\cos^2 l}{6}\sin^3 y. \qquad \text{(F)}$$

Hence, in measurements to a certain extent, x and y may be regarded as equal; and either of them will give the amplitude of the length measured. It is easy to substitute y for x in the expression of the geodetical line already given.

It is requisite to observe that in low latitudes the value of $\sin y \left(= \frac{\tan w}{\tan l}\right)$ is the quotient of two small quantities; and that an error in ϖ will be greatly augmented in y. It is therefore only in considerable latitudes that z can be safely determined by means of the azimuth.

The foregoing analysis will enable us to deduce the difference of longitude directly from the variation in azimuth. We have already found,

$$\sin\varpi = \frac{\sin l\sin z}{\sqrt{\cos^2 l + \sin^2 l\sin^2 z}} \times \frac{1}{\sqrt{1 + e^2\cos^2 l}};$$

therefore assume

$$\sin\theta = \frac{\sin w\sqrt{1 + e^2\cos^2 l}}{\sin l};$$

then

$$\sin\theta = \frac{\sin z}{\sqrt{\cos^2 l + \sin^2 l\sin^2 z}},$$

$$\frac{\tan z}{\cos l} = \tan\theta.$$

But we have likewise found

$$\varphi = \text{arc}\tan\left(\frac{\tan z}{\cos l}\right) - \cos l\left\{\frac{e^2}{2} - \frac{3e^4}{8}\cos^2 l\right\} \times z:$$

wherefore, we get,

$$\left.\begin{aligned}\sin\theta &= \frac{\sin w\sqrt{1 + e^2\cos^2 l}}{\sin l},\\ \varphi &= \theta - \left\{\frac{e^2}{2}\cos l - \frac{3e^4}{8}\cos^3 l\right\} \times \text{arc tan.}(\tan\theta\cos l).\end{aligned}\right\}\text{(G)}$$

This formula is already very exact, and will extend to an amplitude of 10° or 12° from the beginning of the geodetical line: but the method we have employed may be carried to any required degree of approximation.

In the *Conn. des Tems* 1828, I find an example very proper to illustrate the foregoing calculations. In a perpendicular to the meridian commencing in latitude 45° M. Puissant has computed the angles at the extremity of a length equal to 400,000 metres, supposing the oblateness equal to $\frac{1}{309}$: and the results of his calculation, given in pp. 221, 222, expressed by the symbols we have and are as follows:

$$s = 400{,}000$$
$$l = 45^\circ$$
$$u = 44^\circ\ 53'\ 14''{\cdot}73$$
$$\varphi = 5^\circ\ 4'\ 3''{\cdot}78$$
$$\mu' = 86^\circ\ 25'\ 8''{\cdot}46:$$

also, taking the proportion of the axes of the spheroid, we have $\sqrt{1 + e^2} = \frac{309}{308}$, and hence

$$e^2 = \frac{617}{308^2} = 0{\cdot}0065045$$

log. $-3{\cdot}8131838$.

We may now compare the values of z computed in the different ways we have investigated. In the first place, by the formula, $\sin u = \sin l \cos z$, we get

$$z = 3^\circ\ 35'\ 39''.$$

As u is here the result of an exact calculation and not affected with errors of observation, the value of z now found must be accurate as far as the tables usually employed will allow. But if u were determined by observation, we may reasonably suppose an error of $1''$ in defect; then

$$z = 3^\circ\ 35'\ 54''{\cdot}8,$$

so that an error of $1''$ in u has produced one of near $16''$ in z.

Next computing by the difference of longitude, we have

$$\tan \varphi \cos l = \tan x,$$
$$x = 3^\circ\ 35'\ 17''{\cdot}13;$$

then, by the formula (D),

$$z = x \times 1{\cdot}001631 = 3^\circ\ 35'\ 38''{\cdot}20.$$

Lastly, to determine z by means of the variation in azimuth, we have

$$w = 90^\circ - \mu' = 3^\circ\ 34'\ 51''{\cdot}54$$
$$\sin y = \frac{\tan w}{\tan l},$$
$$y = 3^\circ\ 35'\ 16''{\cdot}78:$$

then, by the formula (E)

$$z = y \times 1{\cdot}001625 + 0''{\cdot}11 = 3^\circ\ 35'\ 37''{\cdot}88.$$

The values of z deduced from the difference of longitude and the variation of azimuth are not exactly equal, the former exceeding the latter by $0''{\cdot}32$, which seems to arise from small errors in the calculated longitude and azimuth. For according to the formula (F) the arcs x and y ought to be more nearly equal than they are found to be. I have likewise computed the difference of longitude φ directly from the azimuth by means of the formula (G), and have found it equal to $5^\circ\ 4'\ 3''\cdot 32$, or $0''\cdot 46$ less than it should be, which agrees with the remark just made.

I shall not prosecute this subject further at present. It would be interesting to investigate the general case of a geodetical line directed in any angle to the meridian; but it would occupy too much room. The relations of all the quantities concerned in the problem have, in the foregoing analysis, been expressed by formulæ so simple and manageable, that there can be little difficulty in the investigation of any point that can occur in practice; and it is in this that I conceive the advantage of the solution I have given to consist.

Since my last communication on this subject, the 41st number of the Quarterly Journal of the Royal Institution has appeared, which contains some investigations of M. Bessel relating to the curve of shortest distance on a spheroid of revolution. It is extremely remarkable that M. Bessel's general solution of the problem is exactly the same with that which I published in this Journal for July, 1824*. By saying this I mean, not that every step of his investigation is the same with mine, but that the same view is taken of the problem, and the ultimate formulæ obtained, are not a jot different from those which I have given. The two formulæ marked (5) in p. 139 of the Journal of Science, are identical with the two marked (A) in my solution, the apparent difference existing only in the notations. Although this is so plain as to require only to be noticed, yet in a case of this kind it may not be improper to prove incontestibly the exact coincidence of the expressions. Now one of my formulæ (A) is this,

$$ds = ds' \sqrt{1 + e^2 \sin^2 \psi},$$

which belongs to a spheroid of which the semi-axis of revolution is unit; and if the same semi-axis be of any other magnitude P, it is evident that we must write $\frac{ds}{P}$ for ds, and then we shall have,

$$ds = P \times ds' \sqrt{1 + e^2 \sin^2 \psi}:$$

put $1 - \cos^2\psi$ for $\sin^2\psi$; then

$$ds = P\sqrt{1 + e^2} \times ds' \sqrt{1 - \frac{e^2 \cos^2 \psi}{1 + e^2}}:$$

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* It is proper to observe that I have no knowledge of M. Bessel's writings on this subject, except from the Journal of Science.

but when the semi-axis of revolution is changed from 1 to P, the equatorial semi-diameter will be changed from $\sqrt{1 + e^2}$ to $P \sqrt{1 + e^2} = a$; and the formula will now be,

$$d s = a \times d s' \sqrt{1 - \frac{e^2 \cos^2 \psi}{1 + e^2}},$$

which is identical with the first of the formulæ (5) in the Journal of Science, because in my notation $d s'$, ψ, and $\frac{e^2}{1 + e^2}$ stand for the same things as $d \sigma$, u, and e^2 in M. Bessel's. The other of my formulæ (A) is,

$$d \varphi = d \varphi' \times \frac{\sqrt{1 + e^2 \sin^2 \psi}}{\sqrt{1 + e^2}};$$

or,

$$d \varphi = d \varphi' \sqrt{1 - \frac{e^2 \cos^2 \psi}{1 + e^2}},$$

which is identical with the second of the formulæ (5). It is therefore certain that the two investigations end in the same results. The equations marked (4) in p. 138 of the Journal are no more than the equations (5) in p. 139 in a different shape. I investigated these equations by giving to the coordinates a certain form, which led to them directly without successive substitutions, and many intermediate inferences: M. Bessel has arrived at the same conclusion by setting out from the usual property of the curve of shortest distance on a solid of revolution, and by a train of reasoning which rests upon properties directly flowing from my analysis. How this coincidence has happened I am not called upon to give any account. The date of my solution exculpates me from the charge of silently producing formulæ found by another, as my own under a little disguise, in the form of the expression and the mode of investigation.

If we except the general solution of the problem, M. Bessel's investigations contain nothing new or of much interest. His principal formula (10) in p. 141, is the length of an elliptic arc expressed in a complicated manner, and requiring in practice bulky tables, and the calculation of many subsidiary arcs. Let us try with what success the methods we have followed will apply to determine the geographical position of places on a given spheroid.

In the first place we have,

$$\cos i = \cos \lambda \sin \mu = \frac{\cos l \sin \mu \sqrt{1 + e^2}}{\sqrt{1 + e^2 \cos^2 l}}:$$

but, from the relation between the arcs i and i', we likewise have,

$$\cos i = \frac{\cos i' \sqrt{1 + e^2}}{\sqrt{1 + e^2 \cos^2 i'}} :$$

wherefore, by equating the two values of cos i, we get,

$$\cos i' = \frac{\cos l \sin \mu}{\sqrt{1 + e^2 \cos^2 l \cos^2 \mu}} . \qquad (1)$$

Again, by combining the formulæ (A) and (B), we get,

$$d s = \frac{d \psi \cos \psi \sqrt{1 + e^2 \sin^2 \psi}}{\sqrt{\sin^2 i - \sin^2 \psi}};$$

but, we have,

$$\sin i = \frac{\sin i'}{\sqrt{1 + e^2 \cos^2 i'}}, \quad \sin \psi = \frac{\sin u}{\sqrt{1 + e^2 \cos^2 u}};$$

wherefore, by substitution,

$$d s = \frac{(1 + e^2) \sqrt{1 + e^2 \cos^2 i'} . d u \cos u}{(1 + e^2 \cos^2 u)^{\frac{3}{2}} \sqrt{\sin^2 i' - \sin^2 u}} :$$

and hence,

$$\sin u = \sin i' \sin z$$

$$d s = \frac{(1 + e^2) \sqrt{1 + e^2 \cos^2 i'} . d z}{(1 + e^2 - e^2 \sin^2 i' \sin^2 z)^{\frac{3}{2}}}$$

This formula is different from that we have obtained above in no other respect, except that s and z are now to be reckoned from the equator, the one along the geodetical line, and the other along the great circle of the inscribed sphere, having the inclination i' to the equator. The terminations of s and z are points in the two lines that have the same latitude u. We next obtain,

$$f^2 = \frac{e^2 \sin^2 i'}{1 + e^2},$$

$$d s = \frac{\sqrt{1 - f^2} . d z}{(1 - f^2 \sin^2 z)^{\frac{3}{2}}}.$$

In order to integrate this formula, I assume,

$$\frac{s}{\sqrt{1 - f^2}} = A z - \cos z \{B \sin z + C \sin^3 z + \&c.\} ;$$

then having taken the fluxions and made the two values of $d s$ coincide, I have found,

$$A - B = 1$$

$$2B - 3C = \frac{3}{2} f^2$$

$$4C - 5D = \frac{3.5}{2.4} f^4$$

&c.

and hence by exterminating B, C, &c. successively, we get,

$$A = 1 + \left(\frac{1}{2}\right)^2 \cdot 3f^2 + \left(\frac{1.3}{2.4}\right)^2 \cdot 5f^4 + \&c.$$

$$B = A - 1$$

$$C = \frac{2}{3}B - \frac{1}{2}f^2 \qquad \&c.$$

The coeficients B, C, &c. decrease at the same rate with the successive powers of f^2. It may be remarked here, once for all, that, if we are to calculate with the usual tables, the approximation need not be carried further than to include f^4: for it will be found that the other terms affect the numbers only in the eighth decimal place, which is beyond the reach of the ordinary tables. This being observed, we have,

$$A^{(0)} = A\sqrt{1-f^2} = 1 + \frac{1}{4}f^2 + \frac{13}{64}f^4$$

$$A^{(1)} = B\sqrt{1-f^2} = \frac{3}{4}f^2 + \frac{21}{64}f^4$$

$$A^{(3)} = C\sqrt{1-f^2} = \frac{15}{32}f^4$$

$$s = A^{(0)}z - \cos z\left\{A^{(1)}\sin z + A^{(3)}\sin^3 z\right\}.$$

Now this very simple expression will accomplish all that can be effected by M. Bessel's formula (10) in p. 141 of the Journal of Science. But it will be more convenient in practice if it be written a little differently, as follows,

$$m = \frac{1}{A^{(0)}} = 1 - \frac{1}{4}f^2 - \frac{9}{64}f^4,$$

$$p = \frac{A^{(1)}}{A^{(0)}} = \frac{3}{4}f^2 + \frac{9}{64}f^4,$$

$$q = \frac{A^{(3)}}{A^{(0)}} = \frac{15}{32}f^4,$$

$$m\,s = z - \cos z\left\{p\sin z + q\sin^3 z\right\}. \qquad (2)$$

For illustration I take M. Bessel's example in pp. 143, 144, of the Journal of Science. The latitude of Seeberg, or l, is 50° 56′ 6″·7; μ, or the azimuth, 85° 38′ 56″·82 reckoning from the north westward; and, if s be the distance from Seeberg to Dunkirk on the geodetical line, and reduced to a spheroid of which the polar semi-axis is unit, we have, $\log s =$ 8·9649485. Further, the square of the excentricity is, in my notation, equal to $\frac{e^2}{1+e^2}$; and according to M. Bessel,

$$\log\frac{e^2}{1+e^2} = 7{\cdot}8108710$$

$$\log e^2 = 7{\cdot}8136900.$$

From these data we get, by the formula (1),

$$l' = 51^\circ\ 4'\ 9''{\cdot}94.$$

And hence,

$$f^2 = \frac{e^2 \sin^2 i'}{1 + e^2} = 0{\cdot}0039150$$

$$f^4 = 0{\cdot}0000153.$$

It will be convenient to multiply the coefficients m, p, q by the seconds in an arc equal to the radius in order to have all the terms of the formula expressed in seconds of a degree: then,

$$\log m = 5{\cdot}3139988,$$

$$\log p = 2{\cdot}78254,$$

$$\log q = 0{\cdot}170,$$

The arc z is the hypothenuse of a right-angled triangle in which the latitude subtends the angle i': therefore,

$$\sin z^{(0)} = \frac{\sin l}{\sin i'};$$

or, when i' and l are very nearly equal, we may use this formula,

$$\text{cot.}\ z^0 = \frac{\sqrt{\sin (i' + l) \sin (i' - l)}}{\sin l};$$

$$z^0 = 86^\circ\ 28'\ 19''{\cdot}0.$$

This quantity must now be substituted in the formula (2) to find $m\, s^\circ$; the amount of the terms to be subtracted is only $37''{\cdot}31$; therefore,

$$m^0 s = 86^\circ\ 27'\ 41''{\cdot}69.$$

In order to get $m\,(s^0 + s)$ we must add the degrees in the arc between Seeberg and Dunkirk, viz. $5^\circ\ 16'\ 48''{\cdot}48$, which is found by adding the log. of m to the log. of the distance between the two places; then,

$$m\,(s^0 + s) = 91^\circ\ 44'\ 30''{\cdot}17.$$

This value being found, we must next compute the corresponding quantity $z^0 + z$ by the formula (2): the correction to be applied is only $-\ 18''{\cdot}4$; wherefore,

$$z^0 + z = 91^\circ\ 44'\ 11''{\cdot}77.$$

Finally, we have,

$$\sin u = \sin i' \times \sin (z^\circ + z);$$

the lat. of Dunkirk, $u = 51^\circ\ 2'\ 12''{\cdot}7.$

In order to find the difference of longitude, I shall resume the expression of $d\,\varphi$ before found, writing i' for l, and $\sin z$ for $\cos z$, and likewise, for the sake of brevity, putting $\Delta^2 = \cos^2 i' + \sin^2 i' \cos^2 z = 1 - \sin^2 i' \sin^2 z$: then,

$$d\,\phi = \frac{\cos i'\, d\,z}{\Delta} \cdot \frac{1}{\sqrt{1 + e^2\, \Delta^2}}.$$

By expanding the radical we get,

$$d\,\varphi = \frac{\cos i'\, d\,z}{\Delta} - \cos i\, d\,z \left\{ \frac{e^2}{2} - \frac{3}{8} e^4\, \Delta^2 + \frac{5}{16} e^6\, \Delta^4 - \&c. \right\}.$$

If now we substitute what Δ^2 stands for, and in place of the powers of $\sin z$ write the equivalent expressions in the cosines of the of the multiples of the arc, we shall get with sufficient exactness,

$$C - \cos i' \left(\frac{e^2}{2} - \frac{3e^4}{8} + \frac{5e^6}{16} - \&c. \right) \cdot$$

$$+ \cos i' \sin^2 i' \left(\frac{3e^4}{16} - \frac{5e^6}{16} + \&c.\right)$$

$$+ \cos i' \sin^4 i' \times \left(\frac{15e^6}{128} - \&c.\right)$$

$$d\varphi = \frac{\cos i'\, dz}{\Delta} - C \times dz + \frac{3e^4 \sin^2 i' \cos i'}{16}\, dz \cos 2z.$$

Now $\int \frac{\cos i'\, dz}{\Delta}$ is the arc φ', or the difference of longitude of the two extremities of the arc z; and hence, by integrating between the limits z^0 and $z^0 + z$, we get,

$$\varphi = \varphi' - C \times z - \frac{3e^4 \cos i' \sin^2 i'}{16} \cos(2z^0 + z) \sin z.$$

Now in the foregoing example, we have

$e^2 \cos i' = {\cdot}0040917$
$e^4 \cos i' = {\cdot}0000266{\cdot}4$
$e^6 \cos i' = {\cdot}0000001{\cdot}7$
$e^4 \cos i' \sin^2 i' = 0000161{\cdot}2$

and hence we get,

$$C = 0{\cdot}0020389,$$

and the log. of the coefficient of the remaining term, multiplied by the seconds in the arc equal to radius, is 9·791. Wherefore, the arc z being $5^\circ\ 15'\ 52''{\cdot}77 = 18952''{\cdot}77$, we get,

$$\varphi = \varphi' - 38''{\cdot}64 - 0''{\cdot}06.$$

The arc φ' is readily computed by this formula,

$$\sin \varphi' = \frac{\cos i' \sin z}{\cos l \cos u}:$$

$$\begin{array}{r} \varphi' = 8^\circ\ 21'\ 57''{\cdot}76 \\ -\ 38{\cdot}7 \\ \hline \varphi = 8\ \ 21'\ 19''{\cdot}06 \end{array}$$

which is the difference of longitude between Seeberg and Dunkirk, the latter place being west of the former.

In such calculations, the defect is not in the algebraic formulæ, but in the tables in ordinary use, which are not sufficient to ensure exactness in the fractions of a second.

The editor of the Journal of Science greatly approves of M. Bessel's researches, and he comments upon them with all that complacency which is so natural to him when he thinks he has got things in a right train. He concludes his remarks with announcing in set phrase, *a simple rectification of the geodelitic curve.* It is an expression of the length of an elliptic arc at which he has arrived with the help of Hirsch's tables. Now there is perhaps no problem in pure mathematics that has more engaged the attention of geometers than the various ways of computing elliptic arcs. In particular Legendre has written largely on this subject, and has published extensive tables for the use of the calculator. Hence there is some difficulty as to the sense in which we are to understand the word *simple*. Is it to be taken generally in reference to the labours of all mathematicians? Or is it merely intended to mark a contrast with the complicated calculations of M. Bessel? This is a point which I shall not take upon me to decide; although I should not be surprised if it shall be found that, on the present as on other occasions, this member of the Royal Institution has outdone, with a stroke of his pen, all that has hitherto been attempted on the same subject. After all it may perhaps be alledged that the word in question slipt in cursorily, and its meaning must not therefore be scanned too precisely. In conclusion should any part of these investigations happen to hit the fancy of the Editor of the Journal of Science, I beg leave to suggest the propriety of his taking it from the pages of this Journal without waiting to get it at second-hand from Germany.

May, 5, 1826. J. IVORY.

LIV. *Character and Description of* Kingia, *a new Genus of Plants found on the South-west Coast of New Holland: with Observations on the Structure of its Unimpregnated Ovulum; and on the Female Flower of Cycadeæ and Coniferæ. By* ROBERT BROWN, *Esq., F.R.S.S.L. & E., F.L.S.*

(*Read before the Linnean Society of London, Nov.* 1 & 15, 1825*.)

IN the Botanical Appendix to the *Voyage to Terra Australis*, I have mentioned a plant of very remarkable appearance, observed in the year 1801, near the shores of King George the Third's Sound, in Mr. Westall's view of which, published in Captain Flinders's Narrative, it is introduced.

The plant in question was then found with only the imperfect remains of fructification: I judged of its affinities, therefore, merely from its habit, and as in this respect it entirely agrees with Xanthorrhœa, included the short notice given of it in my remarks on Asphodeleæ, to which that genus was referred†. Mr. Cunningham, the botanist attached to Captain

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* From Captain King's Survey of the Intertropical and Western Coasts of Australia, 1826, vol. ii. p. 534.

† Flinders's Voyage, vol. ii p. 576.

King's voyages, who examined the plant in the same place of growth, in February 1818 and in December 1821, was not more fortunate than myself. Captain King, however, in his last visit to King George's Sound, in November 1822, observed it with ripe seeds: and at length Mr. William Baxter, whose attention I had particularly directed to this plant, found it, on the shores of the same port in 1823, both in flower and fruit. To this zealous collector, and to his liberal employer Mr. Henchman, I am indebted for complete specimens of its fructification, which enable me to establish it as a genus distinct from any yet described.

To this new genus I have given the name of my friend Captain King, who, during his important surveys of the coasts of New Holland, formed valuable collections in several departments of Natural History, and on all occasions gave every assistance in his power to Mr. Cunningham, the indefatigable botanist who accompanied him. The name is also intended as a mark of respect to the memory of the late Captain Philip Gidley King, who, as governor of New South Wales, materially forwarded the objects of Captain Flinders's voyage; and to whose friendship Mr. Ferdinand Bauer and myself were indebted for important assistance in our pursuits while we remained in that colony.

KINGIA.

Ord. Nat. *Junceæ* prope Dasypogon, Calectasiam et Xerotem.

Char. Gen. *Perianthium* sexpartitum, regulare, glumaceum, persistens. *Stamina* sex, fere hypogyna: *Antheris* basi affixis. *Ovarium* triloculare, loculis monospermis; *ovulis* adscendentibus. *Stylus* 1. *Stigma* tridentatum. *Pericarpium* exsuccum. indehiscens, monospermum, perianthio scarioso cinctum.

Planta *facie* Xanthorrhœæ elatioris. Caudex *arborescens cicatricibus basibusve foliorum exasperatus?* Folia *caudicem terminantia confertissima longissima, figura et dispositione* Xanthorrhœæ. Pedunculi *numerosi foliis breviores, bracteis vaginantibus imbricatis tecti, floriferi terminales erecti, mox, caudice parum elongato foliisque novellis productis, laterales, et divaricati vel deflexi, terminati capitulo denso globoso floribus tribracteatis*

Kingia *Australis.* Tab. C.

Desc. *Caudex* arborescens erectus simplicissimus cylindraceus, 6—18 pedes altus, crassitie femoris. *Folia* caudicem terminantia numerosissima patula, apicibus arcuato-recurvis, lorea, solida, ancipitia apice teretiusculo, novella undique tecta pilis adpressis strictis acutis lævibus, angulis lateralibus et ven-

trali retrorsum scabris. *Pedunculi* numerosi teretes 8—12-pollicares crassitie digiti, vaginis integris brevibus imbricatis hinc in foliolum subulatum productis tecti. *Capitulum* globosum, floridum magnitudine pruni minoris, fructiferum pomum parvum æquans. *Flores* undique densè imbricati, tribracteati, sessiles. *Bractea exterior* lanceolata brevè acuminata planiuscula erecta, extus villosa intus glabra, post lapsum fructus persistens: *duæ laterales* angusto-naviculares, acutissimæ, carina lateribusque villosis, longitudine fere exterioris, simul cum perianthio fructifero, separatim tamen, dilabentibus. *Perianthium* sexpartitum regulare subæquale glumaceum: *foliola* lanceolata acutissima disco nervoso nervis immersis simplicissimis, antica et postica plana, lateralia complicata lateribus inæqualibus, omnia basi subangustata, extus longitudinaliter sed extra medium præcipue villosa, intus glaberrima, æstivatione imbricata. *Stamina* sex subæqualia, æstivatione stricta filamentis sensim elongantibus: *Filamenta* fere hypogyna ipsis basibus foliolorum perianthii quibus opposita leviter adhærentia, filiformia glabra teretia: *Antheræ* stantes, ante dehiscentiam lineares obtusæ filamento paulo latiores, defloratæ subulatæ vix crassitie filamenti, loculis parallelo-contiguis connectivo dorsali angusto adnatis, axi ventrali longitudinaliter dehiscentibus, lobulis baseos brevibus acutis subadnatis: *Pollen* simplex brevè ovale læve. *Pistillum: Ovarium* sessile disco nullo squamulisve cinctum, lanceolatum trigono-anceps villosum, triloculare, loculis monospermis. *Ovula* erecta fundo anguli interioris loculi paulo supra basin suam inserta, obovata lenticulari-compressa, aptera: *Testa* in ipsa basi acutiusculâ foramine minuto perforata: *Membrana interna* respectu testæ inversa, hujusce nempe apici lata basi inserta, ovata apice angustato aperto foramen testæ obturante: *Nucleus* cavitate membranæ conformis, ejusdem basi insertus, cæterum liber, pulposus solidus, apice acutiusculo lævi aperturam membranæ internæ attingente. *Stylus* trigonus strictus, infra villosus, dimidio superiore glabro, altitudine staminum, iisdem paulo præcocior, exsertus nempe dum illa adhuc inclusa. *Stigmata* tria brevissima acuta denticuliformia. *Pericarpium* exsuccum, indehiscens, villosum, basi styli aristatum, perianthio scarioso et filamentis emarcidis cinctum, abortione monospermum. *Semen* turgidum obovatum retusum, integumento (testâ) simplici membranaceo aqueo-pallido, hinc (intus) fere a basi acutiuscula, *raphe* fusca verticem retusum attingente ibique in *chalazam* parvam concolorem ampliata. *Albumen* semini conforme densè carnosum album. *Embryo* monocotyledoneus, aqueo-pallidus subglobosus, extremitate inferiore (radiculari) acuta, in ipsa basi seminis situs, semi-immersus, nec albumine omnino inclusus.

Tab. C. fig. 1. KINGIÆ AUSTRALIS pedunculus capitulo florido terminatus; fig. 2. capitulum fructiferum; 3, sectio transversalis pedunculi: 4, folium: hæ magnitudine naturali, sequentes omnes plus minus auctæ sunt; 5, flos; 6, stamen; 7, anthera antice et, 8, eadem postice visa; 9, pistillum; 10, ovarii sectio transversalis; 11, ejusdem portio longitudinaliter secta exhibens ovulum adscendens cavitatem loculi replens; 12, ovulum ita longitudinaliter sectum ut membrana interna solummodo ejusque insertio in apice cavitatis testæ visa sit; 13, ovuli sectio longitudinalis profundius ducta exhibens membranam internam et nucleum ex ejusdem basi ortum; 14, bracteæ capituli fructiferi; 15, pericarpium perianthio filamentisque persistentibus cinctum; 16, pericarpium perianthio avulso filamentorum basibus relictis; 17, semen.

OBS. I.—It remains to be ascertained, whether in this genus a resin is secreted by the bases of the lower leaves, as in Xanthorrhœa; and whether, which is probable, it agrees also in the internal structure of its stem with that genus. In Xanthorrhœa the direction of fibres or vessels of the caudex seems at first sight to resemble in some degree the dicotyledonous arrangement, but in reality much more nearly approaches to that of Dracæna Draco, allowance being made for the greater number, and extreme narrowness of leaves, to which all the radiating vessels belong*.

OBS. II.—I have placed Kingia in the natural order Junceæ along with Dasypogon, Calectasia and Xerotes, genera peculiar to New Holland, and of which the two former have hitherto been observed only, along with it, on the shores of King George's Sound.

The striking resemblance of Kingia, in caudex and leaves, to Xanthorrhœa, cannot fail to suggest its affinity to that genus also. Although this affinity is not confirmed by a minute comparison of the parts of fructification, a sufficient agreement is still manifest to strengthen the doubts formerly expressed of the importance of those characters, by which I attempted to define certain families of the great class Liliaceæ.

In addition, however, to the difference in texture of the outer coat of the seed, and in those other points, on which I then chiefly depended in distinguishing Junceæ from Asphodeleæ, a more important character in Junceæ exists in the position of the embryo, whose radicle points always to the base of the seed, the external umbilicus being placed in the axis of the inner or ventral surface, either immediately above the base as in Kingia, or towards the middle, as in Xerotes.

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* My knowledge of this remarkable structure of Xanthorrhœa is chiefly derived from specimens of the caudex of one of the larger species of the genus, brought from Port Jackson, and deposited in the collection at the Jardin du Roi of Paris by M. Gaudichaud, the very intelligent botanist who was attached to Captain De Freycinet's voyage.

OBS. III.—*On the Structure of the* UNIMPREGNATED OVULUM *in Phænogamous Plants.*

The description which I have given of the Ovulum of Kingia, though essentially different from the accounts hitherto published of that organ before fecundation, in reality agrees with its ordinary structure in Phænogamous plants.

I shall endeavour to establish these two points; namely, the agreement of this description with the usual structure of the Ovulum, and its essential difference from the accounts of other observers, as briefly as possible at present; intending hereafter to treat the subject at greater length, and also with other views.

I have formerly more than once* adverted to the structure of the Ovulum, chiefly as to the indications it affords, even before fecundation, of the place and direction of the future Embryo. These remarks, however, which were certainly very brief, seem entirely to have escaped the notice of those authors who have since written on the same subject.

In the botanical appendix to the account of Captain Flinders's Voyage, published in 1814, the following description of the Ovulum of *Cephalotus follicularis* is given: "Ovulum erectum, intra testam membranaceam continens sacculum pendulum, magnitudine cavitatis testæ," and in reference to this description, I have in the same place remarked that, "from the structure of the Ovulum, even in the unimpregnated state, I entertain no doubt that the radicle of the Embryo points to the umbilicus†".

My attention had been first directed to this subject in 1809, in consequence of the opinion I had then formed of the function of the Chalaza in seeds‡; and some time before the publication of the observation now quoted, I had ascertained that in Phænogamous plants the unimpregnated Ovulum very generally consisted of two concentric membranes, or coats, inclosing a Nucleus of a pulpy cellular texture. I had observed also, that the inner coat had no connexion either with the outer or with the nucleus, except at its origin; and that with relation to the outer coat it was generally inverted, while it always agreed in direction with the nucleus. And lastly, that at the apex of the nucleus the radicle of the future Embryo would constantly be found.

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* Flinders's Voyage, vol. ii. p. 601, and *Linn. Soc. Trans.* vol. xii. p. 136.

† Flinders's Voy. loc. cit.

‡ *Linn. Soc. Trans.* vol. x. p. 35.

On these grounds my opinion respecting the Embryo of Cephalotus was formed. In describing the Ovulum in this genus, I employed indeed, the less correct term "sacculus," which, however, sufficiently expressed the appearance of the included body in the specimens examined, and served to denote my uncertainty in this case as to the presence of the inner membrane.

I was at that time also aware of the existence, in several plants, of a foramen in the coats of the Ovulum, always distinct from, and in some cases diametrically opposite to, the external umbilicus, and which I had in no instance found cohering either directly with the parietes of the Ovarium, or with any process derived from them. But, as I was then unable to detect this foramen in many of the plants which I had examined, I did not attach sufficient importance to it; and in judging of the direction of the Embryo, entirely depended on ascertaining the apex of the nucleus, either directly by dissection, or indirectly from the vascular cord of the outer membrane: the termination of this cord affording a sure indication of the origin of the inner membrane, and consequently of the base of the nucleus, the position of whose apex is therefore readily determined.

In this state of my knowledge the subject was taken up, in 1818, by my lamented friend the late Mr. Thomas Smith, who, eminently qualified for an investigation where minute accuracy and great experience in microscopical observation were necessary, succeeded in ascertaining the very general existence of the foramen in the membranes of the Ovulum. But as the foramina in these membranes invariably correspond both with each other and with the apex of the nucleus, a test of the direction of the future Embryo was consequently found nearly as universal, and more obvious than that which I had previously employed.

To determine in what degree this account of the vegetable Ovulum differs from those hitherto given, and in some measure that its correctness may be judged of, I shall proceed to state the various observations that have been actually made, and the opinions that have been formed on the subject, as briefly as I am able, taking them in chronological order.

In 1672, Grew* describes in the outer coat of the seeds of many Leguminous plants a small foramen, placed opposite to the radicle of the Embryo, which, he adds, is "not a hole casually made, or by the breaking off of the stalk," but formed for purposes afterwards stated to be the aëration of the Embryo, and facilitating the passage of its radicle in germination. It appears that he did not consider this foramen in the testa as always present, the functions which he ascribes to it being performed in cases where it is not found, either, according to him, by the hilum itself, or in hard fruits, by an aperture in the stone or shell.

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* *Anatomy of Veget. begun* p. 3. *Anat. of Plants,* p. 2.

In another part of his work[*] he describes and figures, in the early state of the Ovulum, two coats, of which the outer is the testa; the other, his "middle membrane," is evidently what I have termed nucleus, whose origin in the Ovulum of the Apricot he has distinctly represented and described.

Malpighi, in 1675[†], gives the same account of the early state of the Ovulum; his "secundinæ externæ" being the testa, and his chorion the nucleus. He has not, however, distinguished, though he appears to have seen, the foramen of Grew, from the fenestra and fenestella, and these, to which he assigns the same functions, are merely his terms for the hilum.

In 1694, Camerarius, in his admirable essay on the sexes of plants[‡], proposes, as queries merely, various modes in which either the entire grains of pollen, or their particles after bursting, may be supposed to reach and act upon the unimpregnated Ovula, which he had himself carefully observed. With his usual candour, however, he acknowledges his obligation on this subject to Malpighi, to whose more detailed account of them he refers.

Mr. Samuel Morland, in 1703[§], in extending Leeuwenhoek's hypothesis of generation to plants, assumes the existence of an aperture in the Ovulum, through which it is impregnated. It appears, indeed, that he had not actually observed this aperture before fecundation, but inferred its existence generally and at that period, from having, as he says, "discovered in the seeds of beans, peas, and Phaseoli, just under one end of what we call the eye, a manifest perforation, which leads directly to the seminal plant," and by which he supposes the Embryo to have entered. This perforation is evidently the foramen discovered in the seeds of Leguminous plants by Grew, of whose observations respecting it he takes no notice, though he quotes him in another part of his subject.

In 1704, Etienne François Geoffroy[||], and in 1711, his brother Claude Joseph Geoffroy[¶], in support of the same hypothesis, state the general existence of an aperture in the unimpregnated vegetable Ovulum. It is not, however, probable that these authors had really seen this aperture in the early state of the Ovulum in any case, but rather that they had merely advanced from the observation of Grew, and the conjecture founded on it by Morland, whose hypothesis they adopt without acknowledgment, to the unqualified assertion of its existence, in all cases. For it is to be remarked, that they take no notice of what had previously been observed or asserted on the more important parts of their subject, while several passages are evidently copied, and the whole account of the original state and developement of the Ovulum is literally translated from Camerarius's Essay. Nor does the younger Geoffroy mention the earlier publication of his brother, from which his own memoir is in great part manifestly derived.

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[*] *Anat. of Plants*, p. 210. tab. 80.

[†] *Anatome Plant.* p. 75 et 80.

[‡] *Rudolphi Jacobi Camerarii de sexu plantarum epistola*, p. 8, 46, et seq.

[§] *Philosoph. Transact.* vol. xxiii. n. 287. p. 1474.

[||] *Quæstio Medica an Hominis primordia Vermis?* in auctoris *Tractatu de Materia Medica*, tom. i. p. 123.

[¶] *Mem. de l'Acad. des Sc. de Paris*, 1711, p. 210.

In 1718, Vaillant[*], who rejects the vermicular hypothesis of generation, supposes the influence of the Pollen to consist in an aura, conveyed by the tracheæ of the style to the ovula, which it enters, if I rightly understand him, by the funiculus umbilicalis: at the same time he seems to admit the existence of the aperture in the coat.

In 1745, Needham[†], and in 1770, Gleichen[‡], adopt the hypothesis of Morland, somewhat modified, however, as they consider the particles in the grains of Pollen, not the grains themselves, to be the embryos, and that they enter the ovula by the umbilical cord.

Adanson, in 1763[§], states the Embryo to exist before fecundation, and that it receives its first excitement from a vapour or aura proceeding from the Pollen, conveyed to it through the tracheæ of the style, and entering the Ovulum by the umbilical cord.

Spallanzani[||], who appears to have carefully examined the unimpregnated Ovula of a considerable variety of plants, found it in general to be a homogeneous, spongy, or gelatinous body; but in two Cucurbitaceæ to consist of a nucleus surrounded by three coats. Of these coats he rightly supposes the outermost to be merely the epidermis of the middle membrane or testa. Of the relative direction of the testa and inner coat in the two plants in question he takes no notice, nor does he in any case mention an aperture in the Ovulum.

Gærtner, who, in the preface to his celebrated work, displays great erudition in every branch of his subject, can hardly, however, be considered an original observer in this part. He describes the unimpregnated Ovulum as a pulpy homogeneous globule, whose epidermis, then scarcely distinguishable, separates in a more advanced stage, and becomes the testa of the seed, the inner membrane of which is entirely the product of fecundation[*]. He asserts also that the Embryo constantly appears at that point of the ovulum where the ultimate branches of the umbilical vessels perforate the inner membrane; and therefore mistakes the apex for the base of the nucleus.

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[*] *Discours sur la Structure des Fleurs*, p. 20.

[†] *New Microscopical Discoveries*, p. 60.

[‡] *Observ. Microscop.* p. 45 *et* 61. § cxviii.

[§] *Fam. des Plant.* tom. i. p. 121.

[||] *Fisica Anim. e Veget.* tom. iii. p. 309--332

In 1806, Mons. Turpin[†] published a memoir on the organ, by which the fecundating fluid is introduced into the vegetable ovulum. The substance of this memoir is, that in all Phænogamous plants fecundation takes place through a cord or fasciculus of vessels entering the outer coat of the ovulum, at a point distinct from, but at the period of impregnation closely approximated to, the umbilicus, and to the cicatrix of this cord, which itself is soon obliterated, he gives the name of Micropyle: that the ovulum has two coats, each having its proper umbilicus, or, as he terms it, omphalode; that these coats in general correspond in direction; that more rarely the inner membrane is, with relation to the outer, inverted; and that towards the origin of the inner membrane the radicle of the embryo uniformly points.

It is singular that a botanist, so ingenious and experienced as M. Turpin, should, on this subject, instead of appealing in every case to the unimpregnated ovulum, have apparently contented himself with an examination of the ripe seed. Hence, however, he has formed an erroneous opinion of the nature and origin, and in some plants of the situation, of the micropyle itself, and hence also he has in all cases mistaken the apex for the base of the nucleus.

A minute examination of the early state of the ovulum does not seem to have entered into the plan of the late celebrated M. Richard, when in 1808 he published his valuable and original *Analyse du Fruit*. The ovulum has, according to him, but one covering, which in the ripe seed he calls episperm. He considers the centre of the hilum as the base, and the chalaza, where it exists, as the natural apex of the seed.

M. Mirbel, in 1815, though admitting the existence of the foramen or mycropyle of the testa[‡], describes the ovulum as receiving by the hilum both nourishing and fecundating vessels[*], and as consisting of a uniform parenchyma, in which the embryo appears at first a minute point, gradually converting more or less of the surrounding tissue into its own substance; the coats and albumen of the seed being formed of that portion which remains[†].

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[*] *Gært. de Fruct. et Sem.* i. p. 57, 59, et [illegible].

[†] *Annal. du Mus. d'Hist. Nat.* vii. p. 199.

[‡] *Elém. de Physiol. Vég. et de Bot.* tom. i. p. 49.

In the same year, M. Auguste de Saint Hilaire[‡] shows that the micropyle is not always approximated to the umbilicus; that in some plants it is situated at the opposite extremity of the ovulum, and that in all cases it corresponds with the radicle of the embryo. This excellent botanist, at the same time, adopts M. Turpin's opinion, that the micropyle is the cicatrix of a vascular cord, and even gives instances of its connexion with the parietes of the ovarium; mistaking, as I believe, contact, which in some plants unquestionably takes place, and in one family, namely, Plumbagineæ, in a very remarkable manner, but only after a certain period, for original cohesion, or organic connexion, which I have not met with in any case.

In 1815 also appeared the masterly dissertation of Professor Ludolf Christian Treviranus, on the developement of the vegetable Embryo[§], in which he describes the ovulum before fecundation as having two coats; but of these, his inner coat is evidently the middle membrane of Grew, the chorion of Malpighi, or what I have termed nucleus.

In 1822, Mons. Dutrochet, unacquainted, as it would seem, with the dissertation of Professor Treviranus, published his observations on the same subject[||]. In what regards the structure of the ovulum, he essentially agrees with that author, and has equally overlooked the inner membrane.

It is remarkable that neither of these observers should have noticed the foramen in the testa. And as they do not even mention the well-known essays of MM. Turpin and Auguste de St. Hilaire on the micropyle, it may be presumed that they were not disposed to adopt the statements of these authors respecting it.

Professor Link, in his *Philosophia Botanica*, published in 1824, adopts the account given by Treviranus, of the coats of the ovulum before impregnation[¶]; and of M. Turpin, as to the situation of the micropyle, and its being the cicatrix of a vascular cord. Yet he seems not to admit the function ascribed to it, and asserts that it is in many cases wanting[**].

[To be continued.]

[*] *Elém. de Physiol. Vég. et de Bot.* tom. i. p. 314.

[†] *Id.* loc. cit.

[‡] *Mém. du Mus. d'Hist. Nat.* ii. p. 270, *et seq.*

[§] *Entwick. des Embryo im Pflanzen-Ey.*

[||] *Mém. du Mus. d'Hist. Nat.* tom. viii. p. 241, *et seq.*

[¶] *Elém. Philos. Bot.* p. 338.

[**] *Id.* p. 340.

LV. *Report made to the Academy of Sciences, 22d of August 1825, on the Voyage of Discovery, performed in the Years 1822, 1823, 1824, and 1825, under the command of M.* DUPERREY, *Lieutenant of the Navy.*

(*Commissioners: MM.* DE HUMBOLDT, CUVIER, DESFONTAINES, CORDIER, LATREILLE, DE ROSSEL; *and* ARAGO, *Reporter.*)

[Concluded from p. 289.]

Tides.

THE observations of the tides, in the rapid navigation of the Coquille, had for their principal object the ascertaining the hour of high-water in different ports. The journals of the expedition contain all the elements of these determinations.

On some coasts M. Duperrey remarked that there was only one tide in the twenty-four hours. Similar observations are recorded in the works of several old navigators; perhaps even now they are sufficiently multiplied for it to be possible to arrive at some interesting conclusion on the local causes which modify so remarkably the general phænomenon. It is an inquiry to which M. Duperrey intends to devote himself.

During the observation of the tides, when the weather was calm, experiments were regularly made on board the Coquille, for the purpose of determining to what depth it is possible to see, where the bottom is of a decidedly white tint: it was in some degree a measure of the transparency of the water. The apparatus employed was composed of a plank two feet in diameter, painted white, and having weights attached in such a manner that in descending in the water it would remain horizontal. The results, as might be expected, were very dissimilar. At Offak, in the isle of Waigiou, in calm and *cloudy* weather on the 13th of September, the disc disappeared when it had sunk to 18 metres (55 feet). The next day, (the 14th,) the sky being clear, we did not lose sight of the same disc till at the depth of 23 metres (70 feet). At Port Jackson, the 12th and 13th of February (it is evident that here the date is of importance), we were never able to see the plank at the depth of more than 12 metres (36 feet) in a dead calm. The mean at New-Zealand, in April, was at a metre less. At the Isle of Ascension, in January, under favourable circumstances, the extreme limits, in a series of eleven experiments, are 28 and 36 feet. We have reported these results, because they belong to interesting questions to which natural philosophers have paid much attention for some years past.

Geo-

*Geological Collection**.

This collection we owe to the care and researches of M. Lesson. It is composed of only 330 specimens; but these specimens have been collected with judgement, and from all the countries which the corvette visited. They are besides of a fine size, and perfectly characterized.

Twelve of these specimens collected in the neighbourhood of Saint Catherine, on the coast of Brazil, show that this part of the American continent belongs to the ordinary granite formations. Thirty-three specimens from the Malouine Isles show that these isles belong to the oldest intermediate formations. M. Lesson found there only phyllades, quartzose sandstones, and grau-wackés, rarely furnishing some organic impressions similar to those known elsewhere. Twenty specimens were collected in the environs of Conception, on the coast of Chili. Some of them, from the peninsula of Talcaguana, are of phylladiform talcose rocks, and consequently belong to the most recent primordial districts. The others, collected on the continent, present ordinary granitic rocks, and besides, true stratiform lignite, which at first sight might be taken for pit-coal. This lignite is dug at Penco; its existence may lead us to presume that there is at this point a portion of a tertiary formation of some extent. Two specimens of grayish phtanite were picked up near Lima; they prove the prolongation of the talcose phylladiform districts in this part of the coast of Peru.

The environs of Payta, on the same coast, have furnished fifty-two very varied specimens. They are: 1. talcose phylladiform rocks, of which, according to M. Lesson's account, the whole country consists, which consequently belongs to the primordial epoch; 2. clays, freestone, and *calcaires grossiers*, which compose a considerable district in which the strata are horizontal. This vast tertiary outlier is placed on talcose rocks 150 feet above the level of the sea; its thickness is 72 feet in the escarpments which M. Lesson visited. Sandy clays traversed by small veins of fibrous gypsum and quartzose sandstones compose the inferior beds. Numerous varieties of *calcaire grossier* form the upper layers. These varieties present most remarkable analogies with several of the varieties of the same rock of the environs of Paris. Their discovery is as curious as it is important.

Twenty-five specimens were brought from two of the Society Islands, namely, Otaheite and Borabora. All the specimens from Otaheite are basaltic lavas well characterized, and not ancient. So for the most part are those of Borabora; the others present a beautiful variety of dolerite.

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* This article is by M. Cordier.

The environs of Port Praslin, in New-Ireland, have furnished seven specimens of a recent madreporitic calcareous rock, similar to that which appears in the formation of nearly all the isles of the Pacific.

At the island of Waigiou, near the land of the Papous, M. Lesson collected twenty-one varieties of serpentine rocks which abound on this spot.

At the Moluccas, the island of Borou furnished six specimens of phylladiform talcite, both carboniferous and quartziferous; and the island of Amboyna afforded four of recent madreporitic calcareous specimens. The specimens collected as well in the countries near Port Jackson as in the Blue Mountains, very much increase our knowledge respecting these parts of New-Holland. The specimens, to the number of seventy, offer us: 1. the granites, the quartziferous syenites, and the pegmatites, which form the second plane of the Blue Mountains; 2. the ferruginous freestones, containing abundant laminæ of specular iron, which cover not only a vast extent of country near the coasts, but also the first plane of the Blue Mountains; and, 3dly, the stratiform lignite which is worked at Mount-York, at 1000 feet above the level of the sea, and the presence of which adds to the reasons, which lead us to think that the ferruginous freestones of these countries belong to the tertiary formation.

Twenty-five specimens collected in Van Diemen's Land, in the neighbourhood of Port Dalrymple, and near to Cape Barren, indicate: 1st, pegmatitic and serpentine districts; 2nd, intermediary shelly formatious, formed of the schistoid grau-wacké, and of calcareous stone; 3rd, very recent districts, composed of sandy and ferruginous clay, with geodes of hydrated iron, and fossil wood in different states. There are also fine white and blueish topazes amongst the quartzose pebbles collected at Cape Barren. Eight specimens from New Zealand, present: 1st, a beautiful variety of obsidian; 2d, scaly basalt passing into phonolite; and 3d, a bright-red tufa, similar to that which appears in so conspicuous a manner in the volcanic mountains of Mezin in France, and in the Giants' Causeway in Ireland. The natives use it to paint their bodies; they also use it to colour their canoes. Finally, the other specimens are volcanic products from the Isle of France, Saint Helena, and Ascension. The rocks of Saint Helena consist of trachytic porphyries; those of Ascension are basaltic, with the exception of a beautiful variety of greenish obsidian, which is chatoyant, like that of Peru.

It appears from these details, that the geological collections of M. Lesson assist in completing the data which we already possess respecting various parts of the vast countries traversed by the expedition; and that they furnish us with new and important documents relative to many points which had not before been ascertained.

*Zoology**.

Captain Duperrey, M. Durville, second-in-command, and MM. Lesson and Garnot, medical officers, who were particularly occupied in zoological researches during the voyage, have hastened to lay before us all the subjects which they have collected, as well as the journals and registers containing their observations. Several of our colleagues of the Museum of Natural History have examined these fine collections along with us; M. Valenciennes, a naturalist of this establishment, has prepared a catalogue of the vertebrated animals, mollusca, and zoophytes which form part of them; and M. Latreille took upon himself the department of the insects, crustacea, and the arachnides. From these materials the account which we shall give is prepared. It was natural that we should state it, not merely to acknowledge our gratitude to those who have assisted us, but also to call in to the aid of our judgement the authority which they possess.

In the first place we must speak of the good state of preservation in which these collections have arrived: it is a merit of the highest importance in Natural History, and one which raises the expeditions of later times incalculably above those which have preceded them.

Experienced naturalists know that repeated observations and accurate comparisons alone can verify the species of an organized being; and when the beginning has not been thus made, all that can be said of this being, of its manners, of its uses, or of the peculiarities of its organization, remain without any basis. The works also which now give the most vexation to naturalists, those which put them sometimes to a sort of torture, are those of travellers who have been obliged, by the circumstances under which they were, to make all their observations during their route, without bringing back or depositing in a known cabinet the objects which they observed. The most laboured descriptions, figures apparently the best-executed, when the objects themselves do not accompany them, are far from being always in a state to satisfy this first want of the science. It happens perpetually, that next in order to a species which was thought well defined by a certain number of characters, there comes another which has the same characters with the first, and which is only distinguished by some features hardly apparent, which the describer, isolated from both, has not thought of noting: if the naturalist cannot see them together, and compare them point by point with the most attentive eye, he will never be able to seize their differences; and yet it is too often on such insufficient data that the most general and the most important doctrines are hazarded,—such as the geography of animals, the limits of their extension, and all the consequences which belong to this order of facts.

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* This article of the Report relative to Zoology, was furnished by M. Cuvier.

Botanists fall less frequently into these inconveniences, because the facility with which vegetables are preserved in herbals has obtained for them, at all times, the means of immediate comparison of the objects of their studies: but it is not the same in Zoology, in which, insects and shells excepted, durable collections cannot be formed without great expense, minute attention, and unconquerable patience.

We cannot then too warmly express the gratitude which is due to the Minister of Marine, who of late has not set on foot any scientific voyage * without including some persons skilled in the preparation of animals, and giving them orders not only to make in each place a general collection of whatever presented itself, but also to deposit them as soon as they returned, in the King's cabinet, where the administration on its part takes the necessary measures for their preservation, and where, placed in the midst of all the objects of the same kind, they offer to the naturalist sure means of fixing, with certainty, and in all the necessary details, their comparative characters. . .

. . . . †

The gentlemen attached as zoologists to the expedition of M. Duperrey were not discouraged by any fatigue. Hunters and fishermen, as well as preparers, they have collected as many objects as could be expected from the number and length of their opportunities: far from being thwarted by their naval companions, as has too often happened to others, they had them all for auxiliaries; independently of M. Durville, they were above all seconded by M. Berard. All they collected has been preserved, in spite of the difficulties in the way of this sort of operation, the heat of the climates they visited, and the little help they obtained from the natives. They have faithfully and without reserve deposited, on their return, their collections, in an establishment consecrated to science, taken in its highest acceptation; an establishment where all naturalists may study them in concurrence with the collectors, as surely no man

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* The time is yet to come when a similar encomium can be justly claimed in this country.—EDIT.

† We omit here several paragraphs on the best methods of making maritime expeditions useful to the progress of natural history.

worthy of the name would have so little delicacy as to publish anything before them, or without their consent. To the specimens they have added detailed notes on the places and time of their collection, on their names in the idioms of various nations, on the uses made of them; they have placed in their journals many observations on the habits of the animals; in short, with a talent that Peron himself found only in the professional artists who were associated with him, they have made finished figures, coloured after the life, or immediately after death: this last attention is of immense advantage with regard to the fishes, *mollusca*, and *zoophytes*, the first of which soon lose their colours, and the others change even their form so much as to be altogether undistinguishable; and indeed, it will be only from the time of Peron, that the *mollusca* and *zoophytes* of the torrid zone will have begun to be really known. The Russian naturalists of MM. Krusenstern and Kotzebue are even to the present time the only ones who share with our French naturalists the honour of having enlarged this new domain of science. But we must not limit ourselves to this general view; and it is necessary, in order fully to do justice to our zoologists, to enter into the detail of the materials which they have procured for natural history.

All which concerns vertebrated animals has been principally collected by MM. Lesson and Garnot; they also gave much attention to shells, the *mollusca*, and *madrepores;* but M. Durville was principally engaged in attending to insects and other small articulated animals.

The history of the human species occupied their first attention: they procured the skulls of various races, as far as the duty of not wounding the respect of these people for the tombs of their fathers permitted them. Amongst others they have brought those of a people little known of the interior of New Guinea, named *Alfourous.*

The class of quadrupeds could not furnish them with many large species, since they did not stay long on extensive lands. They have brought but twelve; but in this number there is one, the *black* rabbit of the Malouine Islands, which appears new to science; another, the great speckled *Phalangista*, which was not in the Museum of Natural History, and two or three which are found there only in bad condition. Two skulls of the species of dolphin *à scapulaire blanc*, which Peron described, but brought no specimen of, are also an interesting acquisition for our anatomical collections.

The birds are much more numerous. There are 254 species, and of several, four, six, or eight specimens. Of the 254 species, 46 seem to be new to the science; that is to say, not yet described by any naturalist; some, although described, were wanting in the collections of the Royal cabinet: all are interesting from their rarity and beauty; and according to the intentions of the Minister of Marine, those which are not wanted for the cabinet of the King will be sent to ornament those which are forming in the ports.

We shall notice among the most remarkable, a *Barita* with metallic lustre as brilliant as that of the *Calibé* of Buffon, and which sings better than the other species. Our zoologists have taken care to bring its trachea. One motive which led to the choice of New Guinea for one of the principal objects of the voyage, was to observe the Birds of Paradise in their native climate and in their natural state. These gentlemen, indeed, killed some on the tops of the high trees which they inhabit, and have brought them back in a state of perfect preservation. They have amongst others, a female, of which but one incomplete specimen was yet known, in a cabinet in Holland. The *Prion* of M. de Lacépède and the *Vaginalis* of Latham are also among the rare genera; of which we had very few specimens in Europe, and for a fine series of which we shall be indebted to this expedition.

The number of species of reptiles is sixty-three, fifteen or twenty of which at least, will probably be new, and of which nearly a fourth was wanted in the Museum. Amongst others, there is a *Python* from New Holland, nearly seven feet long.

But it is in the class of fishes, above all, that the collection of MM. Lesson and Garnot is abundant. They brought in spirits 288 species, almost all in number and in state of preservation very remarkable, although they did not take away their intestines, which makes them doubly valuable; more than eighty of the number are certainly new; and as they are studied, others will probably be found so. It will be allowed that it is not possible on a first sight to pronounce on a class where the nomenclature is so difficult.

But that which M. Lesson has done of the greatest merit for ichthyology, is having drawn more than 70 of these fish in their natural colours: it is a service rendered to science, even with regard to known species, which for the most part have been described in Europe from specimens discoloured by drying, or by the spirituous liquors in which they were brought. Many of these figures have surprised us by the difference which they show between the supposed colours and those of nature. In having them engraved in colours, like those of the artists of M. Freycinet's expedition, the minister proceeds to furnish ichthyology, with a kind of materials of which it has been too much in want till now; for it is known that even in the famous work of Bloch, the figures of exotic fish are nearly all falsely coloured. We shall notice amongst the most interesting of the fish which our zoologists have brought home, the *Squalus Philippi*, of which there were only the jaws remarkable for their teeth spirally disposed; a new genus of the family of eels, allied to *Sphagebranchus*; the *Macolor*, a singular fish, known only by the work of Renard, and which is of the genus *Diacope*. Their collection will also have the merit of throwing light on the history of many fish, of which there were only descriptions without figures in the manuscripts of Commerson and of Forster.

M. Lesson has shown no less discernment in painting the *Mollusca* after life. His figures will form a valuable continuation to those given by Péron, and those which MM. Quoy and Gaimard have begun to publish. They represent more than 150 of these *Mollusca* or *Zoophytes*, of which a considerable number are of the greatest beauty, either by the diversely ramified tentacula which they display, or by the brilliancy or variety of their colours.

Nevertheless, our naturalists have not neglected to preserve, as much as they were able, these *Mollusca* and *Zoophytes*. If the contractions and discolorations which they undergo do not permit us to contemplate them in all their beauty, we have at least an opportunity of observing the principal features of their structure, and nearly all which is of import to be known of their interior organization.

The species thus preserved in spirits extend to more than fifty, of which twenty at least are entirely new to us: such are the *Glaucus*, the animal of the *Concholepas*, an *Anatifa* nearly without shell, which will make a new genus allied to *Otion*. The shells amount to nearly 120 species, of which fifty are univalves; the [illegible] amongst others a *Monocero*, remarkable for its great size and its long shape. Amongst the zoophytes preserved in spirits, [illegible] great number of *Holothuriæ* are to be remarked for their size and the fine preservation of their colours. There are also several *Echini* and *Asteriæ*, and an *Isis Hippuris* still covered with its crust of polypes, which proves how near this coral approaches the gorgons.

As we have already said, it is principally to M. Durville that we owe the rich collection of insects which form a part of the produce of this expedition. This skilful naval officer undertook in a manner this labour by supererogation, and only devoted himself to it in the moments of leisure which his principal duties left him. The present also which he has made of his insects to the Museum may be regarded as an act of pure generosity. On a former occasion, at the time of his

voyage

voyage in the Black Sea with Captain Gauttier, he studied the interests of the Museum; but in this voyage he has proved still further his zeal and disinterestedness. The insects which he has deposited there amount to nearly 1,200, forming nearly 1,100 species: viz. 361 coleoptera, 428 lepidoptera, and the rest included in the other orders. M. Latreille is of opinion that, of this number, 450 species at least were wanting in the Museum of Natural History, and that nearly 300 are not yet described in published works. They come from Chili, from Lima and Payta in Peru, and more especially from Port Praslin in New-Ireland, from Offak in the land of Papous, from Dory in New-Guinea, from Bourou in the Moluccas, from Otaheite and from the Malouines. Although the Museum already possessed a great number of these animals from New-Holland and Brazil, it will yet acquire by this voyage several species in which it was deficient, and which inhabit exclusively these countries.

M. Lesson had also formed a collection of insects, from which M. Durville has chosen all those which had escaped his search. It is also to the zeal of M. Lesson, seconded by M. Garnot, that the Museum will owe sixty *Crustacea*, natives of the seas which they have traversed, and of which some are new.

A special praise due to the officers whose labours we have just made known, is, that like true naturalists, they have collected every thing; to the smallest species, to those even which they might have supposed to have been common on our coasts; they have not imitated so many voyagers, who, aiming to make a choice and to bring nothing but what seemed remarkable to them, neglect precisely that which would have been interesting. We repeat it, because it cannot be too much repeated to voyagers, The most learned naturalist when he sees an isolated species, is not able to say if the species is new; it is only by having under his eye the series of adjoining species, that he can make sure of its characters. Thus, those are in a great error who, on a voyage, employ themselves with any thing else than collecting means of study, either by preparation, or the drawing of things that preparation cannot preserve; or lastly, in committing to writing all the fugitive circumstances which the object does not carry along with it, and who lose their time in making descriptions or investigations respecting nomenclature, which must always be done over again when the naturalist arrives at his cabinet. It is in accordance with these views, that those engaged in this last expedition have directed and œconomized their activity. There only remains then for them, in order to fulfil, as much as in them lies, the wishes of naturalists, to obtain from Government the means of publishing their discoveries with promptitude and in a manner worthy of the nation for the honour of which they have laboured *.

Botany.

In the division which the officers belonging to the expedition of M. Duperrey made among themselves of the various subjects of inquiry in which they were to be engaged, M. Dumont-Durville of course undertook botany. The rich collections of plants and insects which he brought, in 1820, from the Greek archipelago and the Black Sea, showed what might be expected from his zeal and experience. Although M. Durville, in quality of second-in-command of the corvette, was obliged to pay attention, in port, to all the minute details respecting provisions; although the care of the fittings-out, &c. formed also an anxious part of his duties, this officer,—thanks to the good order which constantly reigned in the Coquille,—was able, without the service suffering, to reconcile the duties of his rank with scientific inquiries. The moist regions of the Malouines; the burning Silla of Payta; the islands of Otaheite and Borabora, the plains of Bathurst on the other side of the Blue-Mountains; the archipelago of the Carolines, were successively the object of his exploration. The herbal which he has brought back is composed of nearly 3000 species; of this number, there are thought to be about 400 new. Several others, although already known, are rare, and are not to be found in the collections of the Museum of Natural History.

Moreover, M. Durville was not satisfied with collecting the plants which presented themselves to his sight; he has analysed and described them with care. Those whose too delicate organs could not be preserved, were drawn on the spot with great success by M. Lesson. The particular flowers of the different countries where the Coquille stopped will show in what numerical relations the families, the genera, and the species are distributed. For example, we see, not without surprise, that, in an extent of more than 4000 leagues, in the whole *intertropical* zone, from the Isle of France as far as Otaheite and much further, on the islands as well as on the continents, the vegetable kingdom presents a great number of identical species; whilst the islands of Saint Helena and Ascension, also situated under this zone in the Atlantic Ocean, produce species which are peculiar to them, and which are not found either in Brazil or in Africa, in the same latitudes.

M. Durville having an intention to observe as much as possible the degree of frequency belonging to each vegetable species, in all the districts which he traversed, will thus have furnished valuable data to those who devote themselves particularly to botanical geography. The notes with which his herbals are accompanied, on the uses of certain species of plants in domestic œconomy, on the nature and elevation of the soil where they grow, on the names which they bear in the different islands, are not less curious. Let us add, that during his voyage M. Durville sent to the Museum several packets of seeds, —the specimens raised from which are now cultivated there. The numerous objects collected and observed by this officer will notably extend the domain of natural science, and ensure him the gratitude of all those who cultivate it.

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* Here ends the article by M. Cuvier.

Historical Relation.

The documents brought by the expedition respecting the manners and habits of the different people of the Carolines, the natives of New Zealand, the inhabitants of Otaheite, so different now from those found there by Cook and Bougainville, have appeared full of interest to us. The vocabularies of the languages of these islands which M. Duperrey has collected are very numerous. We owe some to the personal investigations of our travellers. The greatest number was communicated to them by the English missionaries. These vocabularies will excite to the highest degree the curiosity of those who seek to know how the migration of people was accomplished in the vast extent of the South Sea. We shall be indebted to M. Gabert, mercantile agent, to whom the European languages have become familiar, for curious information on the state of commerce and industry of the colonies visited by the Coquille. As to the physical features of the inhabitants of these different archipelagos, they are represented in a series of 43 portraits executed with a great deal of talent, by the aid of optical means, by M. Lejeune. The resemblance, according to the unanimous testimony of the officers of the Coquille, is more perfect than has ever been obtained by other methods. We are also indebted to M. Lejeune for 57 drawings of costumes; 40 little pictures; 83 views or landscapes; and 59 drawings representing arms, household utensils, and other different objects. The author of this rich portfolio embarked in the Coquille only as an amateur. An artist by profession and with a salary, could hardly have shown, as we see, more zeal and activity. No one will doubt the great advantage which will be afforded by many of these drawings to embellish the historical narrative of this voyage, when we have announced that General Lejeune will consent to become the guide of his nephew in this undertaking. M. Berard, whose activity we have so often had to praise, has drawn with remarkable success all the kinds of canoes which are used by the inhabitants of the numerous archipelagos of the South Sea. It is a work complete in its kind, and which furnishes more than one occasion to admire to what degree necessity and long experience may supply the place of scientific knowledge.

Conclusion.

The Academy will find, in the preceding analysis, a proof that the voyage of the Coquille deserves to occupy a distinguished rank amongst the most brilliant scientific expeditions performed either by the French navy or those of other nations. The Commission has but one wish to express; that a speedy and full publication may put the learned world in possession of riches as numerous as varied, which we owe to the zeal, the talent and the indefatigable activity of M. Duperrey and his fellow-labourers.

LVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 27.—A PAPER was read, entitled, Experiments on the Elasticity of Ice; in a letter from Benjamin Bevan, Esq., to Thomas Young, M.D. For. Sec. R.S.

A paper was also read, On the application of the Floating Collimator to the Dublin Circle; by John Brinkley, D.D. F.R.S. Andrews' Professor of Astronomy, Dublin.

May 4.—A paper was read, On the means of facilitating the observation of distant stations, in Geodesical Operations; by Lieut. T. Drummond, Roy. Eng.: communicated by Lieut. Col. T. Colby, F.R.S.

The means of connecting distant stations in geodesical operations described in this paper, were devised, in order to expedite the prosecution of the new trigonometrical survey of Ireland, undertaken last year, in consequence of a recommendation to Government, to that effect, from a select committee of the House of Commons. Their immediate objects, are, to obviate the delay frequently occurring, from the unfavourable state of the weather rendering the ordinary signals indiscernible, and to render night-observations readily available. For the first object, Lieut. Drummond has invented, and employed with perfect success, an instrument on the principle of Gauss's Heliostat described in the paper, to which illustrative drawings of it are annexed. For the purpose of effecting observations during the night, he has constructed an instrument, in which a spherule of quick-lime is exposed to the flame of alcohol urged by oxygen gas, in the focus of a parabolic reflector. The lime, under this treatment, when the experiment is made in the most perfect manner, emits a light 83 times as intense as that given out by the brightest part of the flame of an Argand lamp; and this, concentrated and reflected by the mirror, has enabled the officers employed in the survey, to connect very distant stations in the night-time, in the most satisfactory manner.

May 11.—A paper was read, On the production and formation of Pearls; by Sir E. Home, Bart., V.P.R.S.: and the reading was commenced of a paper, On the burrowing and boring Marine Animals; by Edward Osler, Esq.; communicated by L. W. Dillwyn, Esq. F.R.S.

The Society then adjourned to May 25.

LINNÆAN SOCIETY.

May 2.—Read a paper On the Locusts (*Gryllus migratorius*, Linn.) which devastated the Crimea and the southern provinces of Russia, in 1824. By J. Smirnove, Esq., F.L.S. Secretary to the Russian Embassy. Also a paper on Indian *Annonaceæ*, by H. T. Colebrooke, Esq., F.R. & L.S.

May 25.—This day, being the birthday of Linnæus, the Anniversary was held as usual, Sir J. E. Smith, President, in the Chair, when the following Fellows were chosen as Officers and Council for the ensuing year:—

President: Sir James Edward Smith, M.D. F.R.S., &c. —*Vice-Presidents*, Samuel Lord Bishop of Carlisle, L.L.D. V.P.R.S. F.A.S.; A. B. Lambert, Esq. F.R.S. A.S. & H.S.; W. G. Maton, M.D. F.R.S. & A.S.; and Edward Lord Stanley, M.P. F.H.S.—*Treasurer*, Edward Forster, Esq., F.R.S. & H.S.—*Secretary*, James E. Bicheno, Esq.—*Assistant Secretary*, Richard Taylor, F.S.A. Mem. Asiat. S.—Also, to fill the vacancies in the *Council*: Charles Bell, Esq. F.R.S. Ed.; John Bostock, M.D. F.R.S. Pres. Geol. Soc.; Sir Stamford Raffles, F.R.S.; Joseph Sabine, Esq. F.R.S. & A.S. Sec. Hort. Soc.; and N. A. Vigors, Esq. M.A. F.R.S.—After the dinner, (which was held at Freemasons' Tavern,) many interesting observations were made by the President and other gentlemen, relative to the progress of Natural History, among which we would mention a recommendation from Mr. Greenough, that the subject of Fossil Plants should occupy the attention of Botanists; as it seemed to be considered neutral ground between the Linnæan and the Geological Society, and had therefore been neglected by both.

GEOLOGICAL SOCIETY.

May 5.—The reading of Dr. Bigsby's paper, on the Geology of the Valley of the St. Lawrence, was concluded.

In the first part of this paper, the author describes the general form of the country, in which are placed the great lakes of Superior, Huron, Erie, and Ontario, with a sketch of some of the rocks occurring in their vicinity.

He then examines the question of the present level of their waters, as compared with their ancient level, and enumerates many circumstances to prove that no recent alteration has taken place. He then endeavours to establish, that the Canadian lakes are monuments of the last flood, by the features of the country, the abrasion of its rocks, and the nature of the transported matter. He ascribes the shape of the Islands of Lake Superior, and of the Promontory of Keweewoonan to diluvial denudation. The Manitouline gaps are adduced as effects of one simultaneous deluge.

Dr. Bigsby divides the debris of the St. Lawrence Valley into four classes:—1. Diluvial; 2. Messalluvial: 3. Alluvial; 4. Native.

1. The Diluvium lies usually in extensive flattened heaps. Cape Tourment, 1800 feet above the level of the sea, is covered with it. Marine shells, of the genus *Saxicava*, are found in the Ottawa, 300 miles N.N.W. of Montreal.

The actual position of numerous primitive bowlders on the plains, being south and south-east of their original sites, indicates that the flood proceeded from the north and north-west, or, in a direction *contrary* to the present course of the St. Lawrence. The trap of Montreal is found at Lake Champlain, and bowlders of tabular spar (one of which weighs 600 pounds) are traced to the west end of Ontario, where the Ophicalcic rock is also seen in broken masses. The chalcedonies from Lake Superior have been transported south-west to Lake Pepin. The south and west shores of the Lake of the Woods are loaded with bowlders, whilst the opposite shore is destitute of them. These evidences of denudation are given, as coinciding with the views of Saussure, De Luc, and Buckland.

2. The *Messalluvion* is presumed to have been formed in the intermediate state of the earth, which it assumed between its total submergence and its present form. At that period, central North America is imagined to have been occupied by one great lake; and the author's evidences to prove this, consist of, 1. The series of embankments, and 2. their being composed of adjacent rocks, and even of fresh-water materials. 3. Rolled masses of neighbouring lakes being reciprocally found in each other. 4. The peculiar nature of the sand and gravel, beneath the mould of the Valley of the St. Lawrence. 5. The mountain-barriers broken through for the passage of rivers and lakes. 6. The analogy of this supposed reservoir to those

which have been traced in Germany, Scotland, &c. This enormous lake, or rather insulated portion of the ocean, must have extended in the north, from Hudson's Bay to below Quebec: the eastern boundary being the Allighany range: the western the diluvial hills near the Rivers St. Peter, Red, and Missouri: whilst the waters contained therein must have stood at 1000 feet above the level of the sea.

That the fluid of this great reservoir was saline, is inferred from many genera of fish, *of marine origin*, being now the inhabitants of the lakes; which latter are presumed to have been converted into fresh-water by various operations of drainage, &c. Large fresh-water deposits are instanced, as occurring on Lakes Huron and Simcoe, extending to Ontario and Erie. Some of the higher beds of these, in the interior, contain *Uniones*, like those of the present lakes. they are never in a fossil state, and are associated with *Planorbes, Physæ, Lymneæ, Melaniæ*, &c. The banks of the lakes are usually constituted of several steps or terraces, which the author attributes to the various depressions of the waters, occasioned by excessive injuries to the embankments; but with respect to the great primary lake, he inclines to the belief of its reduction to a group by one great disruption. The chaudieres, or pot-like cavities, are described in many situations, and the fluted channellings of various rocks are further adduced, as exhibiting the abrasive power of water.

The 3d class, or the alluvial depositions, offer nothing remarkable.—The 4th class, or the native debris, is derived from the disintegration of the subjacent, or primitive rocks. The Nipissing, Lake Huron, &c. offer many examples, the materials composing which appear never to have travelled far, but always to have been derived from the contiguous rocks, unaltered in their outline and angles.

The paper concludes by the description of a limestone cave near New Lanark, in Upper Canada, containing bones of various quadrupeds, the larger of which are supposed to have been carried in by a smaller animal, and of the effects of whose *teeth* there are evident marks.

May 19.—A paper was read, entitled, Notes on the Geological Position of some of the Rocks of the North-East of Ireland; by Lieut. Portlock, R. Eng. F.G.S.

ASTRONOMICAL SOCIETY.

April 14.—At this meeting there was read "A Comparison of Observations made on Double Stars." Communicated in a letter to J. F. W. Herschel, Esq. Foreign [illegible] to this Society, by Professor Struve, of Dorpat. [illegible]ng himself to

Mr. Herschel, M. Struve, says, "You may easily imagine with what interest I have perused the work on double stars, by yourself and Mr. South, and with what pleasure I found that, independently of one another, we have arrived at the same results and deductions. Although my instruments were formerly inferior to yours, with respect to measurements (as I could only observe differences of Æ on the meridian, and angles of position with a 5-feet telescope of Troughton), they may be considered in an optical point of view equal to yours; viz. the 5-feet telescope of Troughton to yours attached to the 5-feet equatorial; and the 8-feet one of Dollond to yours attached to the 7-feet equatorial; and after receiving the repeating micrometer of Fraunhofer, which I fixed to Troughton's telescope, every desideratum in this instrument was fulfilled."

M. Struve, however, found himself involved in some practical difficulties, until the arrival of Fraunhofer's large refractor, an instrument which, with respect to double stars, left him nothing further to wish; and he determined on a new examination of all the double stars observed before (whether by Sir W. Herschel, Messrs. Herschel and South, or himself), as well as on a minute inquiry of the heavens from the north pole to −15° of declination, with respect to these objects. He has now accomplished one-third of the labour, and has found 1000 double stars of the first four classes; *among which* 800 *are new*, and of these nearly 300 are of the first class. He extends the examination to all stars of the 8th and (8·9) magnitude.

The author, after detailing a few more preliminary remarks, enters into a comparison of many of his observations with those of Sir W. Herschel, and of Messrs. Herschel and South, pointing out many cases in which their coincidence is truly remarkable;—others in which there are discrepancies, evidently attributable to the relative or real motions of the stars in the intervals between the observations;—others in which the diversities seem occasioned by the instruments employed;—and others in which there are anomalies which do not, as yet, admit of explanation. This part of M. Struve's communication is not susceptible of abridgement.

Report of the Committee appointed by the Council of the Astronomical Society, for the purpose of examining the Telescope constructed by Mr. Tulley *by Order of the Council.*

Your Committee, in making this report, before entering on the immediate subject of it, think it will not be unsatisfactory to the Council if they recapitulate briefly the circumstances which have led to it, by way of presenting in one view the history of the telescope in question.

So long ago as the 29th of September 1821, a communication was made to the Council of the Society, by M. Reynier, of Neufchatel in Switzerland, on the part of an artist of the name of Guinand, resident in that neighbourhood, stating him to be in possession of a process for making discs of flint glass, fit to be employed in the construction of object-glasses for achromatic telescopes, and free from the defects which have hitherto given so much annoyance to opticians—and of any required size even as far as 12 inches and upwards—and claiming for him a priority of invention before his former employers Messrs. Fraunhofer and Utzschneider of Benedictsbeurn, in case of dispute.

The extreme difficulty experienced by our artists in procuring discs of flint glass of even very moderate dimensions, had long been severely felt, and the first prospect of an opening afforded for the cessation of this inconvenience, which bore so heavily on the progress of practical astronomy, could not but excite the attention of the Council. Declining, however, to constitute themselves judges in any dispute respecting the invention of the process, they contented themselves with inviting M. Guinand to submit specimens of his performance for examination, and making such further inquiries as to his prices and his ability to furnish a regular supply, as the nature of the case and the wants of British artists appeared to call for.

This invitation was at first, however, very unsatisfactorily complied with, the specimen sent being merely sufficient to authorize a judgement as to the quality of the glass with respect to refractive and dispersive power, but too small to justify any conclusion as to the real merit of the process by which it had been made. The principal was a disc of about two inches in diameter, of which an object-glass was immediately constructed by Mr. Tulley, and of which that artist and Mr. Dollond reported as favourably as the trifling magnitude of the specimen would permit. The inquiries of the Council too were answered in a manner hardly more satisfactory, M. Guinand appearing chiefly desirous of disposing of his stock of discs on hand, and that a very limited one, at a tarif annexed, and of obtaining a pecuniary compensation for his secret, rather than of continuing the manufacture. A Committee was therefore appointed, consisting of Messrs. Gilbert, Herschel, and Pearson, to examine the telescope, and thereon to report on the propriety of purchasing a larger specimen for further trial.

The report of this Committee will be found on the books of the Society. (A copy is subjoined.)

A copy of this report was immediately forwarded to M. Guinand, through M. Reynier. Meanwhile, however, other and larger specimens, consisting of fragments of irregular figure, were transmitted; and finally, a disc of 7¼ English inches in diameter was placed by Messrs. Guinand and Reynier, at the absolute disposal of the Society, to examine without reserve, and to report on as its merits should appear to require. It is this disc which forms the chief object of the present report, the fragments, though considerable and apparently of good glass, being still not large enough to excite much interest, or call for particular attention. This, however, was not considered the case with the disc, (one of such size fit for use, being very uncommon, if not at that time unique in England,) and having now, from the free and unreserved mode of its communication, the means of putting the pretensions of the artist to a fair practical test, it was considered by the Council as a duty, due from them to the public, to take every adequate step for that purpose. The disc was, therefore, placed in the hands of Messrs. Dollond and Tulley on the 14th of November 1823, with directions to take every proper means for ascertaining its efficiency for optical uses; and it was finally agreed between those gentlemen, that Mr. Tulley should undertake to form it into the concave lens of an achromatic object-glass of 12 feet focal length.

This has accordingly been done; but considerable difficulty was experienced in obtaining a disc of crown glass sufficiently homogeneous to match it; and it is evident that this was essential to the object in view, both glasses being of equal importance. A disc of French plate glass at that time in his possession was first tried; but after working it with all possible care, the combination turned out defective, and the telescope resulting, though not a bad one, proved inferior to the high expectations which had been formed of it. An artist of ordinary perseverance would, perhaps, have been discouraged by this indifferent success in a trial on so large a scale, and the glass, without further examination, would have been condemned; but Mr. Tulley, with a zeal and constancy for which he is entitled to much credit, still conceiving that the fault might mainly lie in the plate glass, resolved on commencing anew. Having, after much research, obtained another fit disc of the less refractive medium, being English plate glass, he again set to work, *ab initio*, refiguring the flint glass,—and the object-glass now to be reported on is the result of these his second labours. These circumstances your Committee think it necessary to mention by way of accounting (and in their minds satisfactorily)

torily) for the long interval elapsed from the first reception of the disc to the final completion of the object-glass.

In the state in which it has been submitted unreservedly to their inspection, at Mr. Tulley's house at Islington, mounted in a temporary wooden tube, and on a stand of very convenient construction for astronomical uses, its clear aperture is six inches and eight tenths, and its performance has proved in the highest degree satisfactory. It has been tried by us on various objects, both by day and by night; among the latter, the planets Jupiter and Saturn, several of the most delicate and difficult double stars, such as *Polaris*, γ *Leonis*, ζ *Cancri*, ω^2 *Leonis*, &c. as well as some of the small resolvable nebulæ in the constellation *Virgo;* severe tests these of the performance of a telescope, under magnifying powers from 200 to 700.

The examination of a bright object on a dark ground, as a card by day-light, or Jupiter by night, with high magnifying powers, affords, as is well known, the severest test of the perfect achromaticity of a telescope, by the production of green and purple borders about their edges in the contrary case. The telescope in question bears these tests remarkably well, and is certainly more achromatic than usual; a circumstance depending not merely on the nice adjustment of the foci, but on the quality of the flint glass mainly. This might not have been expected (according to a remark of Dr. Brewster) from the high refractive and dispersive power of the glass, but the fact is undoubted.

The destruction of the aberration of sphericity in an object-glass, when thoroughly accomplished, even with the best materials, is the strongest proof of the goodness of its workmanship; but except the materials be good, no excellence of workmanship will destroy that irradiation which surrounds the image of a star with lines of light darting from it as a centre, and which fills the field with loose dispersed light. The object-glass in question is perfectly free from the latter defect, and almost entirely from the former. The rudiments of rays may, indeed, be traced in interruptions of the regular contour of the rings which surround the spurious discs of large stars, and which arise from the interference of the rays grazing the edge of the aperture. Portions of these rings are wanting or very faint, and other portions somewhat stronger; so that in some directions the outlines of rings of several orders may be traced, in others only those of the first and second. This defect was distinctly perceived in the image of γ *Leonis*, with a power of 220, giving it the appearance of having an excessively faint small star, almost close to the large one of the double star; but the inconvenience is so slight, that without critical attention, its existence would not be suspected. It is no way offensive, and can certainly not be called a serious defect, and might arise from imperfect adjustment. In every other respect the definition of this star, with the power mentioned, was excellent.

ω^2 *Leonis* is one of the most difficult double stars in the heavens. With 220 it was seen elongated; with 700 it was distinctly seen to consist of two discs in apparent contact. With this high power, a slight degree of diffusion in the light of the stars was perceptible, but on the whole the performance of the telescope was extremely good.

ζ *Cancri* was examined with 300, 450, and 700. With the lowest power it was seen triple, very beautifully defined, and the close stars distinctly separated. With 450 they were well separated, and the black interval distinctly seen. With 700 the separation remained perfectly distinct.

A minute star was suspected near α^2 *Cancri;* but on comparing the diagrams made of it with its real position, it could not have been the true companion of that very difficult double star, which to be perceived requires the full power of reflectors of the largest class.

The companion of *Polaris* was of course perfectly well seen.

The light of this telescope is, however, amply sufficient for showing the nebulæ of Sir W. Herschel's 1st class. Several of these were examined, and the high degree of concentration of the rays in the focus arising from the absence of aberration proved very valuable, and was evidently marked in the resolvable appearance exhibited by them.

Saturn was shown with great distinctness, the division of the ring, and the three interior of the old five satellites being plainly seen. A satellite on the body of Jupiter was also seen as well as its shadow; and the planetary discs of the other satellites could not be mistaken for spurious ones.

Your Committee consider that the facts above detailed, speak sufficiently for themselves, as to the excellence of the telescope, to render comments or praise on their part, superfluous; but they cannot close this report without observing once more on the great pains bestowed on its workmanship by Mr. Tulley, and his address in availing himself of the resources of his art in operating on a material which might certainly in the beginning be regarded as highly unpromising.

(Signed) G. Dollond.
J. F. W. Herschel.
Wm. Pearson.

Report alluded to above.

At a meeting of the Committee of the Astronomical Society for reporting on the propriety of purchasing specimens of M. Guinand's glass for further trials, held March 17, 1823,

The telescope constructed by Mr. Tulley was produced and examined, and his letter, and that of Mr. Dollond read, as also such parts of a correspondence between the Foreign Secretary and M. Reynier, as appeared necessary.

It was then resolved, That it appears to your Committee that M. Guinand has not answered in a satisfactory manner to the inquiry put to him through M. Reynier, whether he will engage to furnish the London artists at a reasonable price, with flint glass fit for their purposes, inasmuch as he holds out no assurance of a regular supply, and has actually but a very limited quantity of his glass to dispose of, and that principally in discs not exceeding four inches in diameter; and your Committee conceive that no degree of excellence in individual specimens would authorize them to recommend their purchase by the Society, unless supported by such assurances of constant supply, as would render it a matter of public interest.

(Signed) Davies Gilbert.
J. F. W. Herschel.
April 11, 1823. Wm. Pearson.

ROYAL INSTITUTION OF GREAT BRITAIN.

April 28.—A paper on the porphyry of Christiania was read by Mr. S. Solly, in the Lecture-room, and illustrated by a series of engravings and of geological specimens from Prof. Esmark. Instruments, drawings and diagrams, were exhibited and explained in the Library by Mr. Jopling, in illustration of his septenary system of lines produced by double continuous motion. A series of types, stereotype plates, and impressions of type-music-printing from the office of Mr. Clowes, were laid upon the table. The advantages of this mode were stated to be accuracy in composition and clearness of the page.

May 5.—The relations of sulphuric acid to hydro-carbon, as illustrated by the late researches into the nature of the sulphovinic and sulpho-naphthalic acids were detailed by Mr. Faraday, in an experimental discourse from the lecture-table, and the striking points discovered by Mr. Hennell and himself explained and enforced. Mr. Perkins's specimens of patterns produced by eccentric-lathe-turning, and also a pair of his steel plates and rollers for bank-note engraving, were laid on the library table.

May 12.—Lieut. Drummond's beautiful and intense Station light for geodesical operations, was exhibited in the Reading-room, its nature, and arrangements chemical and mechanical, having been previously explained in the Lecture-room by Mr. Faraday. For an account of this light, see our report of the proceedings of the Royal Society.

May 19.—Mr. Turrell read the first part of a practical essay upon steel-engraving, illustrating as he proceeded by numerous specimens of steel, steel plates, tools, specimens of art, &c. An impression from the fine mezzotinto on steel of Martin's Belshazzar's Feast, was hung up in the room. It is the largest specimen of steel engraving that has yet been executed. A new and very pretty photometer was exhibited in the Library by Mr. Ritchie, of Nain. Its principle is that of a comparison of lights equalized by distance, but this is done in a new and very commodious way. The instrument has been described in a paper read to the Edinburgh Royal Society. A Burmese law was brought forward by Mr. Howship, and well illustrated the height to which that nation has attained, in the arts of writing, decoration, &c.

VACCINE POCK INSTITUTION.

We regret having to announce the dissolution of this establishment, on Tuesday, March 21, 1826, at No. 44, Broad-street, Golden-square. Dr. Pearson, F.R.S., &c., delivered the following address to the Board on this occasion.

Gentlemen,—The First Meeting of the Members of this Institution was holden on December 2, 1799, and on this day, Tuesday, March 21, 1826, we are assembled on the occasion of its dissolution.

In justice to the Medical Officers and Managers, and for the satisfaction of the Subscribers who have supported this establishment, it is considered right to give a brief statement of the conduct and transactions since its foundation.

In December 1799, a small body of Medical Practitioners founded the Original Vaccine Pock Institution, with the object of acquiring knowledge by practice of the newly proposed inoculation of the cow pock as vicarious of the small pox, the satisfactory instances, from experience, being at that period extremely limited.

Subscriptions to the amount of about 200*l.* per annum were found sufficient to defray the expenses in the execution of this design. The physicians, surgeons, and apothecaries, not only serving gratuitously, but contributing equally with other subscribers pecuniary assistance.

Although the great object of this society was not to produce the highest possible number of patients, but to investigate carefully the effects of the vaccine contagion, yet a register has been kept reporting the history of nearly 15,000 patients attending at this institution,—a record, it is believed, far exceeding in observations that of the practice of any other society in existence.

But this institution has been still more extensively useful by the diffusion of the practice through other channels. From this source many thousand vaccinators have received matter for inoculation. It has been the appointed office exclusively for the supply of the army, navy, and ordnance departments; and at a very early period, with the approval of the English minister of state, Paris first experienced the benefit of the new practice, by the communication with this institution; as well as, it is believed, Vienna and other parts of Germany, Russia, Portugal, the East and West Indies, America, and New South Wales.

Further, the institution has promoted the practice by affording instructions and showing the mode of inoculation to a number of students and inexperienced vaccinators.

As evidence of the labour of investigation, the Medical Board have issued several publications to promulgate facts and determine doubtful points in practice, and information has been transmitted to several medical and philosophical journals.

Communications have been received and registered of extraordinary occurrences in the new practice; by these means materials have been gradually accumulated in the journals of the institution, which may prove beneficial to mankind by a history of the vaccine inoculation.

On the subject of the maintenance of this institution it will create surprise, and perhaps be found unexampled, that a small body, chiefly of medical men, should be enabled, with the assistance of their friends, to pay the cost of a house, secretary, medicine, printing, postages, advertisements, &c. &c. and even defray the expense of carriage-hire to procure the attendance of extraordinary cases, without incurring debt, during more than twenty-six years.

The number of patients applying for vaccination has been progressively and rapidly increasing, especially during the last three years, as the records of the institution accurately demonstrate, and thereby prove the great advantage the public were weekly deriving from the gratuitous services of the members of this establishment.

It is grievous, however, to state, that owing to deaths of most of the original friends, and defalcations of others by various accidents, and perhaps also by the failure of the institution to solicit the public for contributions, the funds are exhausted,

hausted, and the remaining members of this parent institution now witness the painful event of its dissolution.

Gentlemen,—However painful our feelings, excited by being compelled this day to dissolve the parent vaccine institution, after subsisting more than 26 years, we have, I trust, a source of consolation from our conduct. Posterity will do justice to the spirit, liberality, and industry of men, who, from their own pecuniary resources and individual friends, have performed the great work above represented:—private emolument has been no consideration. The members have aspired to the reward of the honourable estimation of the public, and the enjoyment of the unspeakable pleasure of mitigating the sufferings of humanity.

May the greatest earthly happiness be long enjoyed by you, is the last affectionate wish of your faithful friend and colleague, GEORGE PEARSON.

The total number of patients vaccinated from 1799 to this day, are 14,473.

The number of patients vaccinated of late years, has been at the rate of 1,400 annually.

The following statement shows the expense of maintaining the institution from its commencement, and for the last seven years.

Cash received, from its establishment to the present time	£4309	15	2
Money received in last seven years .	857	16	9¼
Stock sold out in last seven years .	377	9	2
	£1235	5	11¼
Expended in last seven years . .	1327	16	7½

ZOOLOGICAL SOCIETY.

We are happy to be able to announce the complete organization of this Society, the establishment of which has been for some time in contemplation. A meeting of the friends of the Society took place at the rooms of the Horticultural Society, on the 29th of April, at which upwards of a hundred noblemen and gentlemen were present. Among them we noticed,

The Marquess of Lansdowne; Lords Darnley, Egremont, Gage, Auckland, Stanley, Clinton; the President of the Board of Control; the President of the Royal Society; the Right Hon. the Lord Mayor; Sir Thomas Dyke Ackland; Sir Robert Inglis; Sir Everard Home; Sir R. C. Fergusson; Sir Stamford Raffles; the Hon. Mr. Twisselton Fiennes; General Thornton; Dr. Goodenough; Mr. Wm. Hamilton; Mr. H. T. Colebrooke; Mr. Children, of the British Museum; Mr. Duncan, of the Ashmolean Museum, Oxford; Mr. P. Duncan, Ditto; Mr. Lambert; Mr. Marsden; Mr. Sotheby; the

Rev. Mr. Benson; Mr. Vigors; Dr. Harwood; Dr. Horsfield; Mr. Barnard; Mr. Clift; Mr. Murchison; Capt. De Capel Brooke; Dr. Waring; Mr. Stephens; the Rev. Mr. Rackett; Mr. Haworth; Mr. Griffiths; the Rev. Mr. Hope, &c. &c. &c.

Sir Stamford Raffles having been called to the chair, a series of resolutions were proposed, and passed unanimously, for the organization of the Society; and the following President, Council and Officers appointed:

Sir Stamford Raffles, Pres. LL.D. F.R.S. &c.

His Grace the Duke of Somerset, LL.D. F.R.S. &c.
Most Noble the Marquis of Lansdowne, F.R.S. &c.
Right Hon. the Earl of Darnley, F.R.S. &c.
Right Hon. the Earl of Egremont, F.R.S. &c.
RightHon.Visc.Gage,M.A.&c.
Right Hon. Lord Auckland.
Right Hon. Lord Stanley, M.P. V.P.L.S. &c.
Sir E. Home, Bt. V.P.R.S.&c.
E. Barnard, Esq. F.L.S. &c.
J.E. Bicheno,Esq.,Sec. L.S.&c.
J. G. Children, Esq. F.R.S. &c.
H. T. Colebrooke, Esq. F.R.S. &c.
Rev. Dr. Goodenough, F.R.S. &c.
G. B. Greenough, Esq. F.R.S. &c.
MajorGen. Hardwicke,F.R.S. &c.
Thos. Horsfield, M.D., F.L.S. &c.
Joseph Sabine, Esq., F.R.S., *Treasurer.*
Chas. Stokes, Esq., F.R.S. &c.
N. A. Vigors, Esq., M.A., F.R.S. &c., *Secretary.*
C. Baring Wall, Esq.,M.P.&c.

The President then read an opening address to the meeting, explanatory of the past and present state of zoology in this country, and of the objects and views of the Society. We hope to have it in our power to present our readers with this address in a future Number.

In addition to the members mentioned above, as present at the first meeting, the Society already numbers among its most zealous supporters the following distinguished personages:—

H. R. H. the Duke of Sussex, the Dukes of Somerset, Northumberland, and Bedford; the Marquesses of Hertford, Salisbury, and Stafford; Earls Caernarvon, Caledon, Gower, Hardwicke, Lonsdale, Malmsbury, Mountnorris, Minto, Spencer, Stanhope, Winchelsea, Oxford, and Grosvenor; Viscount Dudley and Ward, Viscount Gage; the Bishops of Bath and Wells, London, and Carlisle; Lords Calthorpe, Clifton, Downe, Ducie, Ellenborough, Levison Gower, Holland, Lovaine, and Selsey; Right Hon. C. Arbuthnot, Right Hon. Sir C. Long, Right Hon. Sir G. Rose, Right Hon. Robert Peel, Right Hon. Sir J. Leach, Right Hon. the Lord Mayor, Right Hon. John Beckett, Right Hon. F. C. Robinson; Hon. Col. Bligh, Hon. G. Agar Ellis, Hon. Capt. Percy, Hon. Wm. S. Ponsonby,

sonby, Hon. R. Stopford; Hon. and Rev. Dr. Wellesley; Sir H. Bunbury, Sir C. H. Coote, Sir S. Graham, Sir R. Heron, Sir B. Hobhouse, Sir W. Jardine, Sir J. Shelley, Sir G. T. Staunton, Sir J. Croft, Sir F. Baker, Sir Thomas Lawrence, Sir W. F. Middleton, Sir W. Rawson, Sir P. C. Silvester; Admiral Sir C. Pole, Sir J. E. Smith, Sir H. Halford; John Wilson Croker, Esq. M.P.; Alexander Baring, Esq. M.P.; Richard Heber, Esq.; the Rev. Dr. Goodall; the Rev. William Kirby; Francis Chantrey, Esq.; Alexander MacLeay, Esq.; William Sharpe MacLeay, Esq.; the Dean of Carlisle, &c. &c. &c.

LVII. *Intelligence and Miscellaneous Articles.*

OPPOSITION OF MARS TO THE SUN, OBSERVED BY DR. BURNEY, AT GOSPORT.

AT one o'clock A.M. May 5th, the distance of ♂ from α ♎ was 1° 34′ 45″, and from β ♎ 8° 35′. From these data the right ascension of Mars was $14^h\ 47^m\ 25^s$, from which if we subtract the sun's right ascension, viz. $2^h\ 46^m\ 1^s$, the remainder is $12^h\ 1^m\ 24^s$, the difference of their right ascensions, corresponding nearly with their opposition. But in order to obtain the time of their opposition, it is necessary to reduce their right ascensions to some later hour: taking therefore seven hours later, that is at 8 A.M. May 5th, the right ascension of Mars was $14^h\ 46^m\ 59^s$, and the sun's right ascension at that time was $2^h\ 46^m\ 59^s$, leaving the right ascension of Mars from the sun's right ascension 12 hours, which constitutes their opposition at that hour.

The latitude of Mars, as deduced from his meridional altitude near midnight of the 4th and 5th, are respectively 0° 23′ N. and 0° 20′ 20″ N.

From the interposition of clouds for several nights, this was the only good opportunity that presented itself here of ascertaining the position of Mars, to find the time of his opposition to the sun.

DR. BURNEY ON THE LATE LUMINOUS ARCH.

The luminous arch, or bow of light, that was observed in the evening of the 29th of March, at Jedburgh, Hawick and Kelso in Roxburghshire, and at Carlisle, was not seen at Gosport; nor does it appear to have been visible to observers south of Cumberland. If the *aurora borealis* originates from a modification of electrical effluvium towards the polar regions, which is the generally received opinion, in that case we doubt not, from its appearances here in the years 1817, 1818, 1819, and 1821, and the recent descriptions of the luminous arch, which at long intervals of time accompanies it, that the luminous arch is not formed by reflection, the sky being clear of vapours at the time, but by strong emanations from the densest part of the *aurora*.

The difference in the intensity of light of the *aurora* and the luminous arch, may arise from a difference in the density of the atmosphere through which these electrical emanations have to pass: and as the height of the electrical streams has often been found by trigonometrical measurement considerably above the allowed height of the earth's atmosphere, when formed into a luminous arch; it may therefore appear to distant observers to be bent from its rectilinear course, by passing through media of different densities.

By way of illustrating the arched form of the luminous arch, it is necessary to mention an atmospherical phænomenon of parallel bands of *cirrostratus* (like the meridians on an artificial globe), that sometimes emanate from a dense cloud by the impulse of a freshening wind: these apparently equidistant bands of vapour, which overspread the whole hemisphere, from half to three-quarters of a mile in height, are blown in tolerably straight lines from a dark circular cloud in the wind's eye, or in that part of the horizon from whence the wind proceeds; yet from their height in the concavity of the northern hemisphere, they all appear as regular arcs of a great circle.

As it is probable the *aurora borealis* is produced by the agency of electricity, we may suppose that it is thus formed. In the cold northern regions, when the upper stratum of the atmosphere has become highly electrified by the union of winds of a different temperature and moisture, or by the ascending heat from the earth, and the consequent mixture of atmospherical gases at certain seasons of the year, the quantity of electric fluid collected by these means may be sufficient to expand itself several degrees in extent, and be conducted by humid vapours into the lower medium of the atmosphere, till it meets with a stratum of comparatively dry air that resists its descent: in this state of resistance it may be so violently agitated as to appear to distant observers in very varied forms and colours; and in the case of a redundant quantity, strong emanations from this subtil fluid may produce a luminous arch.

LIST OF NEW PATENTS.

To William Wood, of Summer Hill Grove, Northumberland, for his apparatus for destroying the inflammable air or fire-damp in mines.—Dated the 22d of April 1826.—6 months allowed to enrol specification.

To John Petty Gillespie, of Grosvenor-street, Newington, Surrey, for his new spring or combination of springs for the purpose of forming an elastic resisting medium.—25th of April.—6 months.

To Samuel Brown, of Eagle Lodge, Old Brompton, Middlesex, for his improvements on his former patent dated December 4, 1823, for an engine for effecting a vacuum, and thus producing powers by which water may be raised and machinery put in motion.—25th of April.—6 months.

To Francis Halliday, of Ham, in the county of Surrey, esquire, for his apparatus for preventing the inconvenience arising from smoke in chimneys, which he denominates a wind guard.—25th of April.—6 months.

To John Williams, of the Commercial Road, Middlesex, for improvements on ships' hearths and apparatus for cooking by steam.—27th of April.—2 months.

To William Choice, of Strahan Terrace, and Robert Gibson, of White Conduit Terrace, Islington, for improvements in machinery for making bricks.—27th of April.—2 months.

To Charles Kennedy, of Virginia Terrace, Great Dover Road, Surrey, surgeon, for his improvements in the apparatus used for cupping.—29th of April.—6 months.

To John Goulding, a citizen of the United States of America, but now residing in Cornhill, for his improvements in the machines used for carding, stubbing, slivering, roving, or spinning wool, cotton, flax, or any fibrous materials.—2d of May.—6 months.

To Arnold Buffurn, late of Massachusetts, in the United States of America, but now residing in Jewin-street, hat manufacturer, and John MacCardy, of Cecil-street, Strand, for improvements in steam-engines, communicated from abroad. 6th of May.—6 months.

To Sir Robert Seppings, knight, surveyor of the navy, of Somerset-House, for improvements in the construction of fids, or apparatus for striking top-masts and top-gallant-masts in ships.—6th of May.—6 months.

To William Fenner, of Bushell Rents, Wapping, for improvements in apparatus for curing smoky and cleansing foul chimneys.—6th of May.—6 months.

To Alexander Allard de la Court, of Great Winchester-street, London, esquire, for a new instrument, and improvements in certain well known instruments, applicable to the organ of sight.—6th of May.—6 months.

To Joseph Schaller, of Regent-street, for improvements in the construction or manufacture of clogs, pattens, or substitutes for the same.—6th of May.—6 months.

To Edward Heard, of St. Leonard, Shoreditch, chemist, for a new composition for the purpose of washing in sea and other water.—8th of May.—6 months.

To Levy Zachariah junior, of Portsea, Hants. for a combination of materials to be used as fuel.—8th of May.—6 mo.

Results of a Meteorological Journal for April 1826, *kept at the Observatory of the Royal Academy, Gosport, Hants.*

General Observations.

This month has been very dry and fine for the season. Nature has been and is still clad in her richest blooming robes, and the beautifully varied tints of the blossoms of the fruit and other trees, with the verdure of the fields, have not been surpassed in appearance since the year 1822. No disagreeable weather has occurred to prevent agricultural operations, and every advantage has been taken of it in this quarter. On the 18th instant, at midday, a pair of swallows passed by from the S.W., being the first that have appeared here this year.

During the latter part of the month very considerable changes occurred in the temperature of the air, as induced by cold northerly gales and showers of hail; several frosty mornings followed, and the 28th was the coldest day and night since the 30th of March: on that morning, between 5 and 6 o'clock, there was a slight shower of snow, and early in the mornings of the 29th and 30th, a thick hoar-frost appeared on the tops of the houses and in the fields, and ice in the ponds and ditches as thick as a half-crown piece.

The destructive effects of these late frosts on the bloom, and on the young fruit in open situations, after the uniformly mild weather to the 22d, are already perceptible. In consequence of this transition in the weather, the temperature of spring water, from the 20th to the morning of the 27th, decreased 15-100ths of a degree.

Between the 24th and 28th there was a range in the external thermometer of 35 degrees! so that the chilly air fell like the setting in of a second winter. The mean temperature of the external air this month, is three-tenths of a degree less than that of last April; but it is two degrees higher than the mean of that month for the last ten years.

The atmospheric and meteoric phænomena that have come within our observations this month, are two paraselenæ between 8 and 9 P.M. on the 21st; two coloured parhelia at 5 P.M. on the 25th; one anthelion at 3 P.M. on the 28th; one lunar and four solar halos, eight meteors, lightning from the passing clouds in the evening of the 21st; and six gales of wind, or days on which they have prevailed; namely, one from the North, three from the South-west, and two from the North west.

Numerical Results for the Month.

		Inches.	
Barometer	Maximum	30·37,	April 1st—Wind N.W.
	Minimum	29·18,	Ditto 12th—Wind S.W.
Range of the mercury . .		1·19.	

	Inches.
Mean barometrical pressure for the month	30·010
——— for the lunar period ending the 7th inst.. .	30·040
——— for 14 days, with the Moon in North declin.	30·130
——— for 15 days, with the Moon in South declin.	29·950
Spaces described by the rising and falling of the mercury	5·240
Greatest variation in 24 hours	0·970
Number of changes	15·

Thermometer	Maximum	68°,	April 22d.—Wind N.W.
	Minimum	33	Ditto 28th—Wind N.
Range		35	
Mean temp. of the external air		51·90	
——— for 31 days with the Sun in Aries		49·60	
Greatest variation in 24 hours		23·00	
Mean temp. of spring water at 8 o'clock A.M. . . .		49·05	

De Luc's *Whalebone Hygrometer.*

	Degrees.
Greatest humidity of the air .	96 in the evening of the 7th.
Greatest dryness of ditto . . .	42 in the aftern. of the 29th.
Range of the index	54
Mean at 2 o'clock P.M. . . .	58·1
—— at 8 o'clock A.M. . . .	68·0
—— at 8 o'clock P.M. . . .	70·8
—— of three observations each day at 8, 2, and 8 o'clock	65·6

Evaporation for the month	3·05 inch.
Rain in the pluviameter near the ground .	1·00
Rain in ditto 23 feet high	0·90

Prevailing wind, N.W.

Summary of the Weather.

A clear sky, 3; fine, with various modifications of clouds, 14; an overcast sky without rain, 10; rain, 3.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
17	12	29	3	24	20	12

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
6	1½	½	3½	½	4	6½	7½	30

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr. Burney at Gosport, Mr. J. Cary in London, and Mr. Veall at Boston.*

Gosport, at half-past Eight o'Clock, A.M.						Clouds.							Evaporation.	Rain near the ground.	Height of Barometer, in Inches, &c.		Thermometer				Rain.		Weather.		
Days of Month, 1826.	Barom. in Inches,&c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.			Lond. 1 P.M.	Bost. 8½ A.M.	London.			Boston. 8¼ A.M.	London.	Boston.	London.	Boston.	Wind.
																	8 A.M.	Noon.	11 P.M.						
April 1	30·37	39	49·00	66	NW.	1	1	1	...	1	1	1	...	0·030	30·33	30·00	39	51	39	43	...	...	Fair	Cloudy	NW.
2	30·18	47	......	68	W.	...	1	1	...	...	...	1	...	......	30·14	29·68	44	51	50	47	...	·12	Cloudy	Cloudy. Rain *a.m.*	SW.
3	30·20	51	......	74	NW.	1	...	1	...	1	...	...	0·20	......	30·17	29·72	52	62	49	52·5	...	...	Fair	Stormy	SW.
4	30·23	50	......	74	NW.	...	...	1	...	...	...	...	...	......	30·20	29·72	50	56	50	53·5	...	...	Cloudy	Cloudy	W.
5	30·13	53	......	74	W.	...	...	1	...	...	1	...	...	......	30·17	29·67	50	57	49	52	...	...	Cloudy	Fine	W.
6	30·18	55	......	72	W.	...	...	1	...	...	1	...	·35	......	30·16	29·71	52	59	49	56 5	...	...	Cloudy	Fine	W.
7	30·15	53	49·00	92	W.	...	...	1	...	1	...	...	...	......	30·16	29 60	53	61	50	58	...	...	Fine	Fine	W.
8	30·17	54	......	78	W.	...	1	1	1	1	...	...	...	......	30·12	29 70	51	61	53	55	...	...	Fine	Fine	calm
9	29·81	54	......	69	S.	1	1	1	...	1	...	1	·25	·045	29·72	29 60	55	64	45	57·5	...	...	Cloudy	Fine	calm
10	29 96	50	......	72	W.	...	1	1	...	1	1	1	...	·080	29·95	29·40	52	57	50	50	...	...	Fine	Stormy	W.
11	29·87	54	......	74	SW.	1	1	1	...	1	1	1	...	·370	29·80	29·30	55	59	50	57	...	...	Showers	Cloudy	W.
12	29 23	52	......	75	SW.	1	...	1	...	...	1	1	·25	·035	29·22	28·62	50	48	45	52·5	...	·24	Showers	Fine. Rain *a.m.* &	W.
13	30·08	49	......	68	NW.	1	1	1	...	1	1	1	...	......	30·15	29·65	44	55	50	48	...	·4	Fine	Cloudy [*p.m.*	NW.
14	30·28	56	49·10	68	NW.	1	...	1	...	1	...	...	...	......	30·27	29·77	52	61	48	54	...	...	Fine	Fine	W.
☽ 15	30·31	56	......	71	W.	1	...	1	...	1	1	...	·20	......	30·23	29·67	51	55	49	54	...	...	Fine	Fine	NW.
16	30·16	56	......	72	W.	...	...	...	...	1	1	...	...	......	30·18	29·86	48	52	44	50	...	...	Showers	Fine	NW.
17	30 32	48	......	62	N.	...	...	1	...	1	1	...	...	......	30·26	29·85	45	52	43	50·5	...	...	Cloudy	Fine	W.
18	30·25	50	......	63	SE.	1	...	1	...	1	...	...	40	......	30·26	29·65	44	58	43	55	...	...	Fair	Cloudy	W.
19	30·10	56	......	61	S.	1	1	1	1	1	...	...	...	......	30·12	29·53	52	58	44	56	...	...	Cloudy	Fine	calm
20	29·83	54	49·15	60	NE.	1	1	1	...	1	1	1	...	·010	29·78	29 40	48	57	44	57·5	...	...	Fair	Fine	calm
21	29·68	56	......	66	SE.	1	1	1	1	1	1	...	·40	......	29·70	29 35	50	63	50	58	...	...	Cloudy	Fine	calm
○ 22	29·62	58	......	66	E.	...	...	1	...	...	1	1	...	·080	29·71	29·20	58	64	[illegible]	61	...	...	Cloudy	Fine	SE.
23	29·78	48	......	73	N.	...	...	1	...	1	1	...	...	......	29 82	29·32	47	54	41	51	...	·14	Cloudy	Cloudy	W.
24	29·90	47	......	63	N.	1	...	1	...	1	1	...	·30	......	29·92	29·43	43	52	43	47·5	...	...	Cloudy	Fine. Rain *p.m*	W.
25	30·02	48	......	58	N.	1	...	1	...	1	1	1	...	·090	30·00	29·65	42	51	44	49	...	·07	Cloudy	Fine. Rain *p.m.*	NW.
26	29·74	49	49·00	66	NW.	1	1	1	...	1	1	1	...	·180	29·73	29·32	45	52	45	45	...	·06	Fair	Fine	NW.
27	29·46	54	......	63	NW.	...	...	1	...	1	1	1	30	·080	29·50	29·15	43	45	35	45	...	·29	Cloudy	Cloudy. Rain *a.m.*	calm
28	29·90	40	......	57	N.	1	...	1	...	1	...	...	...	......	29·90	29·55	38	44	34	39	...	·05	Fair	Stormy [& *p.m.*	NW.
☾ 29	30·03	43	......	58	N.	1	...	1	...	1	...	1	...	......	30·05	29·77	42	48	35	43·5	...	...	Cloudy	Stormy [stormy	NW.
30	30·17	43	49·05	58	N.	...	1	1	...	1	1	...	·40	......	30·18	29 93	42	50	40	47	...	...	Cloudy	Fine [night.	NW.
Average.	30 004	50 77	49·05	68 0		17	12	29	3	24	20	12	3·05	1·000	30 00	29 55	48	55	45	51·5		1·01			

THE

PHILOSOPHICAL MAGAZINE
AND JOURNAL.

30th *JUNE* 1826.

LVIII. *On the Rectification of Curve Lines.* *By* THOMAS BEVERLEY, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

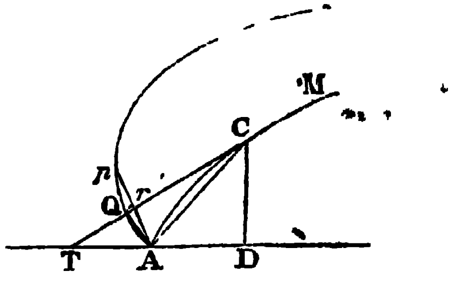
Sir,

THE following property of curves, which I have recently discovered, from its general and extensive application, may probably be found very interesting and useful to those who sometimes make digressions into the more abstruse parts of the higher geometry; as it may be the means of rectifying many curves which have not yet been found susceptible of rectification.

The property is the same in all plane curves whatever, whether they return into themselves, or proceed on *ad infinitum.*

The rectification of the tangential curve is always finite, provided the curve round which it is described either returns into itself or has asymptotes to the infinite branches; if not, it will of course be infinite likewise.

And the rectification is had without even finding the particular equation of the curve to be rectified.



PROPOSITION 1.—ACM is any plane curve, to which CT is a tangent at C, join AC (A being the vertex), and demit AQ perpendicularly on the tangent; so shall the rectification of the curve described by Q be represented by $\int AC \times d.\, TAQ$.

Demonstr.—Draw the ordinate CD, and call it y; also draw the abscissa AD, and call it x. We have (z representing the curve AC), $CT = \frac{y\,dz}{dy}$, $DT = \frac{y\,dx}{dy}$, and therefore by trigonometry $\frac{dx}{dz} = \sin TAQ = \sin TCD$, $\frac{dy}{dz} = \sin \angle T =$

cos TAQ, $AT = \frac{y\,dx - x\,dy}{dy}$, and $AQ = \frac{y\,dx - x\,dy}{dz}$; $\therefore d\,AQ = \frac{1}{dz^2} \times \left\{ (y\,d^2x - x\,d^2y)\,dz - (y\,dx - x\,dy)\,d^2z \right\}$, and $d\,TAQ = \frac{d^2x.dz - d^2z.dx}{dz.dy}$. But since in any case either x or y may be supposed to flow equably, their second fluxions will be equal to zero. Hence, in order to abridge the two last expressions for operation, let us suppose $d^2x = 0$, we shall then have $d\,AQ = -d^2y.dx \times (x.dx + y.dy) \times \frac{1}{dz^3}$, and $d\,TAQ = -d^2y.dx \times \frac{1}{dz^2}$. Draw another polar ordinate Ap, indefinitely near to AQ, and with centre A and radius AQ describe the indefinitely small arc Qr; then is $pr = d\,AQ$, and $Qr = AQ \times d\,TAQ$; and consequently, $Qp = \sqrt{pr^2 + Qr^2}$

$$= \left\{ \left(\frac{-d^2y.dx(xdx+ydy)}{dz^3}\right)^2 + \left(\frac{-d^2y.dx(ydx+xdy)}{dz^3}\right)^2 \right\}^{\frac{1}{2}}$$

$$= \left\{ \frac{d^2y^2.dx^2}{dz^6} \times dz^2(x^2 + y^2) \right\}^{\frac{1}{2}} = \left(\frac{d^2y^2.dx^2(x^2+y^2)}{dz^4}\right)^{\frac{1}{2}}$$

$$= \frac{-d^2y.dx}{dz^2}\sqrt{x^2 + y^2} = AC \times d\,TAQ. \qquad \text{Q. E. D.}$$

Example 1.—Let ACM be a circle whose radius is r, and put $TAQ = 2\phi$; we have $1(= \text{rad.}) : 2r :: \sin\phi : 2r\sin\varphi = AC$, and $\int AC \times d\,TAQ = 4r\int \sin\phi\, d\phi = -4r\cos\varphi$, which, between $\phi = 0$, and 90°, becomes $4r$ for half the length of the curve, whence $2 \times 4r = 8r = 4 \times$ diameter, for the whole length of the *cardioide.*

Example 2.—Now let another curve be similarly described upon the curve we have just investigated, we shall then have to find AQ, and a second angle T′AQ′. $1(= \text{rad.}) : r :: \cos 2\phi : r\cos 2\phi$, and $r - r\cos 2\phi = r(1 - \cos 2\varphi) = 2r\sin^2\varphi = AC$. Also $1(= \text{rad.}) : 2r\sin^2\phi :: \sin 2\varphi : 2r\sin^2\phi.\sin 2\phi = y$, and $1(= \text{rad.}) : 2r\sin^2\varphi :: \cos 2\phi : 2r\sin^2\varphi.\cos 2\varphi = x =$ abscissa of the first *cardioide* $\therefore \frac{dx}{dy} = \frac{2r\,d\varphi \sin 2\varphi(1 - 4\sin^2\varphi)}{4r\,d\varphi\sin\varphi\sin 3\varphi} = \frac{\cos 3\varphi}{\sin 3\varphi} = \cot 3\varphi$, and we therefore obtain $\int AC' \times d\,T'AQ' = 2r\int\sin^2\phi \times 3d\phi = 6r(\frac{1}{2}\varphi - \frac{1}{4}\sin 2\phi) = 3r(\phi - \frac{1}{2}\sin 2\varphi)$, which, when $\varphi = 90°$, becomes $3r \times 1{\cdot}5708 = 4{\cdot}7124\,r$, for half the length of the second *cardioide.*

Remark.—It almost appears from what has already been done, that the angles TAQ, T′AQ′, T″AQ″, &c. will always be equal to $2\varphi, 3\varphi, 4\phi$, &c. and that we shall have $2r\int\sin\phi \times 2d\varphi$, $2r\int\sin^2\varphi \times 3d\varphi$, $2r\int\sin^3\varphi \times 4d\varphi$, &c. for the rectifications of the several curves respectively; but whether it is universal or not I have not yet had leisure to determine.

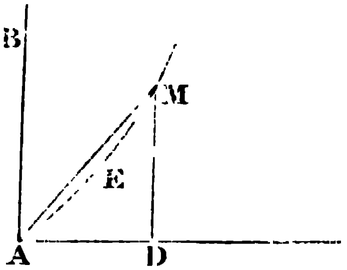
Example 3.—When ACM is the common or *Apollonian parabola,* whose equation is $a x = y^2$, or $y = a^{\frac{1}{2}} x^{\frac{1}{2}}$, a being the parameter. $AC = \sqrt{x^2 + y^2} = \sqrt{a x + x^2}$, $\frac{y d x}{d y} = 2 x =$ subtangent TD, and $\frac{y d s}{d y} = \sqrt{a x + 4 x^2} = CT$, the tangent. Consequently,

$CT : 1 :: DT : \frac{2x}{\sqrt{a x + 4 x^2}} = \sin TAQ$, and $CT : 1 :: CD : \frac{a^{\frac{1}{2}}}{\sqrt{a + 4 x}} = \cos TAQ$; whence $d\,TAQ = \frac{a^{\frac{1}{2}} d x}{x^{\frac{1}{2}} (a + 4 x)}$, and $AC \times d\,TAQ = \sqrt{a x + x^2} \times \frac{a^{\frac{1}{2}} d x}{x^{\frac{1}{2}}(a + 4x)} = a^{\frac{1}{2}} d x \times \frac{\sqrt{a + x}}{a + 4x}$ whose integral is $a^{\frac{1}{2}}\left(\frac{1}{2} \sqrt{a + x} - \frac{1}{4} \sqrt{3 a} \times \text{hyp. log.} \frac{\sqrt{3 a} + 2 \sqrt{a + x}}{\sqrt{a + 4 x}}\right)$.

Example 4.—When ACM is the *cissoid* of *Diocles* whose equation is $y^2 = \frac{x^3}{a - x}$. First $AC = \sqrt{x^2 + y^2} = \sqrt{x^2 + \frac{x^3}{a - x}} = \frac{a^{\frac{1}{2}} x}{\sqrt{a - x}}$; also the subtangent $TD = \frac{y d x}{d y} = \frac{2 x (a - x)}{3 a - 2 x}$, and the tangent $CT = \frac{y d z}{d y} = \frac{a x}{3a - 2 x} \times \left(\frac{4a - 3x}{a - x}\right)^{\frac{1}{2}}$; consequently $CT : 1 :: TD : \frac{2(a - x)^{\frac{1}{2}}}{a \sqrt{4 a - 3 x}} = \sin TCD = \sin TAQ$, and $CT : 1 :: CD : \frac{3 a x - 2 x^2}{a \sqrt{4 a x - 3 x^2}} = \cos TCD = \cos TAQ$; whence $d\,TAQ = \frac{3 d x (a x - x^2)^{\frac{1}{2}}}{4 a x - 3 x^2}$, and $\int AC \times d\,TAQ = \int \frac{a^{\frac{1}{2}} x}{\sqrt{a - x}} \times \frac{3 d x (a x - x^2)^{\frac{1}{2}}}{4 a x - 3 x^2} = \int \frac{3 a^{\frac{1}{2}} x^{\frac{1}{2}} d x}{4 a - 3 x} =$ (when corrected) $\frac{4 a}{\sqrt{3}} \times \text{hyp. log.} \frac{2 \sqrt{a} + \sqrt{3 x}}{\sqrt{4 a - 3 x}} - 2 a^{\frac{1}{2}} x^{\frac{1}{2}}$. And when $x = a$, it becomes $\frac{4 a}{\sqrt{3}} \times \text{hyp. log.} (2 + \sqrt{3}) - 2a = 3{\cdot}041385\, a - 2a = 1{\cdot}041385 a$, or $2 \times 1{\cdot}041385 a =$ the whole length on both sides of the axis.

Example 5.—When ACM is the lemniscata whose equation is $(x^2 + y^2)^2 - a^2 (x^2 - y^2) = 0$. Let $AC = \xi$, and $DAC = \theta$; we have $y = \sin \theta\, \xi$, $(x^2 + y^2)^2 = \xi^4$, and $a^2 (x^2 - y^2) = a^2 (x^2 + y^2 - 2 y^2) = a^2 \xi^2 - 2 a^2 y^2 = a^2 \xi^2 - 2 a^2 \sin^2 \theta\, \xi^2$, whence $\xi^4 = a^2 \xi^2 - 2 a^2 \sin^2 \theta\, \xi^2$ and $\xi = a \sqrt{1 - 2 \sin^2 \theta} = a$

$\cos^{\frac{1}{2}} 2\theta$, which gives $y = a \sin\theta \,.\, \cos^{\frac{1}{2}} 2\theta$, and $x = a \cos\theta \,.\, \cos^{\frac{1}{2}} 2\theta$ $\therefore \frac{y\,dx}{dy} = \frac{-(4\sin^3\theta - 3\sin\theta)\, a \sin\theta \cos^{\frac{1}{2}} 2\theta}{\cos\theta(1 - 4\sin^2\theta)} = a \sin\theta \,.\, \cos^{\frac{1}{2}} 2\theta \times \frac{-\sin 3\theta}{\cos 3\theta}$ = TD, and $a \sin\theta . \cos\frac{1}{2}\, 2\theta$ (TD) : 1 (rad.) : : $a \sin\theta \cos^{\frac{1}{2}} 2\theta$ (y = CD) : $\frac{\cos 3\theta}{-\sin 3\theta} = -\cot 3\theta =$ cot TAQ; therefore DAQ = 3 DAC, and consequently $\int$ AC × d TAQ $= a \cos^{\frac{1}{2}} 2\theta \times 3\, d\theta$, which, by putting $\cos^{\frac{1}{2}} 2\theta = \varphi$, becomes $3a \times \int \frac{\varphi^{\frac{1}{2}}\, d\varphi}{\sqrt{1-\varphi^2}}$, the same as from 5, table 3, Landen's Memoirs (*vide* Math. Repos. vol. i. page 61, old series) by which the whole integral between $\varphi = 1$ (sin 90°), and $\varphi = 0$, is readily found to be $3a \times \cdot 5990701173 = 1\cdot 7972103519\, a$ ∴ the length of the four branches is $7\cdot 1888414076\, a$, a being the axis of the lemniscata.



PROPOSITION 2.—AEM is a plane curve, AB an indefinite axis; join AM, and perpendicular to AB draw AD meeting a perpendicular from M in D; then if the area AEMA have any given ratio to the area AEMD, the curve is of the parabolic kind.

Demonstr.—For put AD $= y$, MD $= x$, and let the ratio of the two areas be as $m : n$; then we must have $\frac{1}{2}$AM$^2 \times d$MAD : MD × dAD : : $m : n$, or $\frac{1}{2}(x^2 + y^2) \times \frac{y\,dx - x\,dy}{x^2 + y^2} : x\,dy : : m : n$, $\therefore \frac{1}{2} n (x^2 + y^2) \times \frac{y\,dx - x\,dy}{x^2 + y^2} = mx\,dy$, and by reduction $\frac{n\,dx}{x} = (2m + n) \times \frac{dy}{y}$. Taking the integrals $n \times$ hyp. log $x = (2m + n) \times$ hyp. log y, and therefore $x^n = y^{2m+n}$, or $a^{2m}\, x^n = y^{2m+n}$, an equation to a curve of the parabolic kind, where a^{2m} is an arbitrary constant. Q.E.D.

Obs.—When the areas are equal, or $m = n$, it becomes $a^2 x = y^3$, the first cubical parabola.

When the areas are as 1 : 2, or $m = 1$, and $n = 2$, it becomes $ax = y^2$, the common or *Apollonian* parabola, &c. &c.

I am, sir, your very obedient servant,

THOMAS BEVERLEY.

Brompton, near Scarborough,
March 6, 1826.

LIX. *On*

LIX. *On the mutual Action of Sulphuric Acid and Naphthaline, and on a new Acid produced. By* M. FARADAY, *Esq. F.R.S. Corresponding Member of the Royal Academy of Sciences, &c. &c.*

[Concluded from p. 332.]

2. *Salts formed by the peculiar Acid with Bases.*

THESE compounds may be formed, either by acting on the bases or their carbonates by the pure acid, obtained as already described; or the impure acid in solution may be used, the salts resulting being afterwards freed from sulphates, by solution in alcohol. It is however proper to mention that another acid, composed of the same elements, is at the same time formed with the acid in question, in small, but variable proportions. The impure acid used, therefore, should be examined as to the presence of this body, in the way to be directed when speaking of the barytic salts; and such specimens as contain very little or none of it should be selected.

Potash forms with the acid a neutral salt, soluble in water and alcohol, forming colourless solutions. These yield either transparent or white pearly crystals, which are soft, slightly fragile, feel slippery between the fingers, do not alter by exposure to air, and are bitter and saline to the taste. They are not very soluble in water; but they undergo no change by repeated solutions and crystallizations, or by long continued ebullition. The solutions frequently yield the salt in acicular tufts, and they often vegetate, as it were, by spontaneous evaporation, the salt creeping over the sides of the vessel, and running to a great distance in very beautiful forms. The solid salt heated in a tube gave off a little water, then some naphthaline; after that a little carbonic and sulphurous acid gases arose, and a black ash remained, containing carbon, sulphate of potash, and sulphuret of potassium. When the salt was heated on platinum foil, in the air, it burnt with a dense flame, leaving a slightly alkaline sulphate of potash.

Soda yields a salt, in most properties resembling that of potash; crystalline, white, pearly, and unaltered in the air. I thought that, in it, the metallic taste which frequently occurred with this acid and its compounds was very decided. The action of heat was the same as before.

Ammonia formed a neutral salt imperfectly crystalline, not deliquescent, but drying in the atmosphere. Its taste was saline and cooling. It was readily soluble in water and alcohol. When heated on platinum foil it fused, blackened, burnt with flame, and left a carbonaceous acid sulphate of ammonia, which by further heat was entirely dissipated. Its general habits were those of ammoniacal salts. When its solutions, though previously rendered alkaline, were evaporated to dryness at common temperatures, and exposed to air, the salt became strongly acid to litmus paper. This however is a property common to all soluble ammoniacal salts, I believe, without exception.

Baryta. It is easy by rubbing carbonate of baryta with solution of the impure acid, to obtain a perfectly neutral solution, in which the salt of baryta, containing the acid already described, is very nearly pure. There is in all cases an undissolved portion, which being washed repeatedly in small quantities of hot water, yields to the first portions a salt, the same as that in the solution. As the washings proceed, it is found, that the salt obtained does not burn with so much flame on platina foil, as that at first separated; and the fifth or sixth washing will perhaps separate only a little of a salt, which when heated in the air, in small quantities, burns without flame in the manner of tinder. Hence it is evident that there are two compounds of baryta, which as they are both soluble in water, both neutral, and both combustible, leaving sulphate of baryta, differ probably only in the quantity of combustible matter present, or its mode of combination in the acid.

It is this circumstance, of the formation of a second salt in small but variable quantities with the first, which must be guarded against, as before mentioned, in the preparation of salts from the impure acid. It varies in quantity according to the proportions of materials, and the heat employed: and I have thought that, when the naphthaline has been in large quantity, and the temperature low, the smallest quantity is produced. When the impure acid is used for the preparation of the salts now under description, a small portion of it should be examined by carbonate of baryta, as above, and rejected, if it furnish an important quantity of the flameless salt.

These bodies may be distinguished from each other provisionally, as the *flaming* and the *glowing* salts of baryta, from their appearances when heated in the air. The latter is more distinctly crystalline than the former, and much less soluble, which enabled me by careful and repeated crystallizations, to obtain both in their pure states.

The *flaming* salt (that corresponding to the acid now under description) when obtained by the slow evaporation of the saturated solution, formed tufts, which were imperfectly crystalline. When drops were allowed to evaporate on a glass plate, the crystalline character was also perceived; but when the salt was deposited rapidly from its hot saturated solution, it appeared in the form of a soft granular mass. When dry, it was white and soft, not changing in the atmosphere. It was readily soluble in water and alcohol, but was not affected by æther. Its taste was decidedly bitter. When heated in the air on platinum foil it burnt with a bright smoky flame, like naphthaline, sending flocculi of carbon into the atmosphere, and leaving a mixture of charcoal, sulphuret of barium, and sulphate of baryta.

After being heated to 212° for some time, the salt appeared to be perfectly dry, and in that state was but very slightly hygrometric. When heated in a tube naphthaline was evolved; but the substance could be retained for hours at a temperature of 500° Fahr. before a sensible portion of naphthaline had separated: a proof of the strength of the affinity by which the hydro-carbon was held in combination. When a higher temperature was applied, the naphthaline, after being driven off, was followed by a little sulphurous acid, a small portion of tarry matter, and a carbonaceous sulphate and sulphuret were left.

This salt was not affected by moderately strong nitric or nitro-muriatic acid, even when boiled with them; and no precipitation of sulphate took place. When the acids were very strong, peculiar and complicated results were obtained. When put into an atmosphere of chlorine, at common temperatures, it was not at all affected by it. Heat being applied, an action between the naphthaline evolved, and chlorine, such as might be expected, took place.

When a strong solution of the pure acid was poured into a strong solution of muriate of baryta, a precipitate was formed, in consequence of the production of this salt. It was re-dissolved by the addition of water. The fact indicates that the affinity of this acid for baryta is stronger than that of muriatic acid.

The *second*, or *glowing* salt of baryta, was obtained in small crystalline groups. The crystals were prismatic, colourless, and transparent: they were almost tasteless, and by no means so soluble either in hot or cold water as the former salts. They were soluble in alcohol, and the solutions were perfectly neutral. When heated on platinum foil they gave but very little flame, burning more like tinder, and leaving a carbonaceous mixture of sulphuret and sulphate. When heated in a tube they gave off a small quantity of naphthaline, some empyreumatic fumes, with a little sulphurous acid, and left the usual product.

This salt seemed formed in largest quantity when one volume of naphthaline and two volumes of sulphuric acid were shaken together, at a temperature as high as it could be without charring the substances. The tint, at first red, became olive green; some sulphurous acid was evolved, and the whole would ultimately have become black and charred, had it not been cooled before it had proceeded thus far, and immediately dissolved in water. A solution was obtained, which though dark itself, yielded, when rubbed with carbonate of baryta, colourless liquids; and these when evaporated furnished a barytic salt, burning without much flame, but which was not so crystalline as former specimens. No attempt to form the glowing salt from the flaming salt by solution of caustic baryta, succeeded.

Strontia. The compound of this earth with the acid already described very much resembled the flaming salt of baryta. When dry it was white, but not distinctly crystalline: it was soluble in water and alcohol; not alterable in the air, but when heated burnt with a bright flame, without any red tinge, and left a result of the usual kind.

Lime gave a white salt of a bitter taste, slightly soluble in water, soluble in alcohol, the solutions yielding imperfect crystalline forms on evaporation: it burnt with flame; and both in the air and in tubes, when heated, gave results similar to those of the former salts.

Magnesia formed a white salt with a moderately bitter taste; crystallizing in favourable circumstances, burning with flame, and giving such results by the action of heat as might be expected.

Iron. The metal was acted upon by the acid, hydrogen being evolved. The moist protoxide being dissolved in the acid gave a neutral salt capable of crystallization. This by exposure to air slowly acquired oxygen, and a portion of persalt was found.

Zinc was readily acted upon by the acid, hydrogen evolved, and a salt formed. The same salt resulted from the action of the acid upon the moist oxide. It was moderately soluble in hot water, the solution on cooling affording an abundant crop of acicular crystals. The salt was white and unchangeable in the air; its taste bitter. It burnt with flame, and gave the usual results by heat.

Lead. The salt of this metal was white, solid, crystalline, and soluble in water and alcohol. It had a bitter metallic taste, with very little sweetness. The results by heat were such as might be expected.

Manganese. The protoxide of this metal formed a neutral crystalline salt with the acid. It had a slightly austere taste, was soluble in water and alcohol, and was decomposed by heat, with the general appearances already described.

Copper. Hydrated peroxide of copper formed an acid salt with the acid, and the solution evaporated in the air left radiated crystalline films. The dry salt when heated fused, burnt with flame, and exhibited the usual appearances.

Nickel. The salt of this metal was made from the moist carbonate. It was soluble, crystalline, of a green colour, and decomposed by heat in the usual manner. In one instance an insoluble sub-salt was formed.

Silver. Moist carbonate of silver dissolved readily in the acid, and a solution, almost neutral, was quickly obtained. It was of a brown colour, and a powerful metallic taste. By evaporation it gave a splendent, white, crystalline salt; not changing in the air except when heated; but then, burning with flame, and ultimately leaving pure silver. When the solution of the salt was boiled for some time, a black insoluble matter was thrown down, and a solution obtained, which by evaporation gave abundance of a yellow crystalline salt. The changes which took place during the action of heat in the moist way were not minutely examined.

Mercury. Moist proto-carbonate of mercury dissolved in the acid forming a salt not quite neutral, crystallizing feebly in the air, white, of a metallic taste, not deliquescent, and decomposed with various phænomena by heat. By re-solution in water or alcohol, and heat, a sub-salt of a yellow colour was formed.

The moist hydrated per-oxide of mercury also dissolved in the acid, forming an acid solution, which by evaporation gave a yellowish deliquescent salt, decomposed by heat, burning in the air, and entirely volatile.

3. *Analysis of the Acid and Salts.*

When solution of the pure acid was subjected to the voltaic battery, oxygen and hydrogen gases were evolved in their pure state: no solid matter separated, but the solution became of a deep yellow colour at the positive pole, occasioned by the evolution of free sulphuric acid, which re-acted upon the hydro-carbon. A solution of the barytic salts gave similar results.

The analytical experiments upon the composition of this acid and its [illegible] were made principally with the compound of baryta. This was found to be very constant in composition, could be obtained anhydrous at moderate temperatures, and yet sustain a high temperature before it suffered any change.

A portion of the pure salt was prepared and dried for some hours on the sand bath, at a temperature about 212°. Known weights were then heated in a platinum crucible to dissipate and burn off the combustible matter; and the residuum being moistened with sulphuric acid to decompose any sulphuret of barium formed, was heated to convert it into a pure sulphate of baryta. The results obtained were very constant, and amounted to 41·714 of sulphate of baryta per cent of salt used, equivalent to 27·57 baryta per cent.

Other portions of the salt were decomposed by being heated in a flask with strong nitro-muriatic acid, so as to liberate the sulphuric acid from the carbon and hydrogen present, and yet retain it in the state of acid. Muriate of baryta was then added, the whole evaporated to dryness, heated red-hot, washed with dilute muriatic acid to remove the baryta uncombined with sulphuric acid, and the sulphate collected, dried, and weighed. The results were inconstant; but the sulphate of baryta obtained, always much surpassed that furnished by the former method. Judging from this circumstance that the sulphuric acid in the salt was more than an equivalent for the baryta present, many processes were devised for the determination of its quantity, but were rejected in consequence of difficulties and imperfections, arising, principally, from the presence and action of so much carbonaceous matter. The following was ultimately adopted.

A quantity of per-oxide of copper was prepared by heating copper plates in air and scaling them. A sufficient quantity of pure muriatic and nitric acids was provided, and also a specimen of pure native carbonate of baryta. Seven grains of the salt to be examined were then mixed with seven grains of the pulverized carbonate of baryta, and afterwards with 312 grains of the oxide of copper. The mixture being put into a glass tube was successively heated throughout its mass, the gas liberated being passed through a mixture of baryta water and solution of muriate of baryta. It was found that no sulphurous or sulphuric acids came off, or indeed sulphur in any state. The contents of the tube were then dissolved in an excess of nitric and muriatic acids, above that required to take up all that was soluble; and a little solution of muriate of baryta was added for the sake of greater certainty. A portion of sulphate of baryta remained undissolved, equivalent to the sulphuric acid of the salt experimented upon, with that contained accidentally in the oxide of copper acids, &c. This sulphate was collected, washed, dried and weighed. Similar quantities of the carbonate of baryta and oxide of copper were then dissolved in as much of the nitric and muriatic acids as was used in the former experiment; and the washings and other operations being repeated exactly in the same way, the quantity of sulphate of baryta occasioned by the presence of sulphuric acid in the oxide, acids, &c. was determined. This deducted from the weight afforded in the first experiments, gave the quantity produced from the sulphuric acid actually existing in the salt. Experiments so conducted gave very uniform results. The mean of many indicated 8·9 grains of sulphate of baryta for 10 grains of salt used, or 89 grains per cent, equivalent to 30·17 of sulphuric acid for every 100.of salt decomposed.

In the analytical experiments, relative to the quantity of carbon and hydrogen contained in the salt, a given weight of the substance being mixed with per-oxide of copper, was heated in a green glass tube. The apparatus used consisted of Mr. Cooper's lamp furnace, with Dr. Prout's mercurial trough; and all the precautions that could be taken, and which are now well known, were adopted for the purpose of obtaining accurate results. When operated upon in this way, the only substances evolved from the salt, were carbonic acid and water. As an instance of the results, 3·5 grains of the salt afforded 11·74 cubic inches of carbonic acid gas, and 0·9 of a grain of water. The mean of several experiments gave 32·93 cubic inches of carbonic acid gas, and 2·589 grains of water, for every 10 grains of salt decomposed.

On these data, 100 grains of the salt would yield 329·3 cubic inches of carbonic acid, or 153·46 grains, equivalent to 41·9 grains of carbon, and 25·89 grains of water, equivalent to 2·877 grains of hydrogen. Hence 100 grains of the salt yielded

Baryta	27·57	78
Sulphuric acid	30·17	85·35
Carbon	41·90	118·54
Hydrogen	2·877	8·13
	102·517	

In the second numerical column the experimental results are repeated, but. increased, that baryta might be taken in the quantity representing one proportional, hydrogen being unity: and it will be seen that they do not differ far from the following theoretical statement.

Baryta	1	proportional	78
Sulphuric acid	2	ditto	80
Carbon	20	ditto	120
Hydrogen	8	ditto	8

The quantity of sulphuric acid differs most importantly from the theoretical statement, and it probably is *that* element of the salt, in the determination of which most errors are involved. The quantity of oxide of copper and of acids required to be used in that part of the analysis, may have introduced errors, affecting the small quantity of salt employed, which when multiplied, as in the deduction of the numbers above relative to 100 parts, may have created an error of that amount.

As there is no reason to suppose that during the combination of the acid with the baryta any change in its proportions takes place, the results above, *minus* the baryta, will represent its composition: from which it would appear, that one proportional of the acid consists of two proportionals of sulphuric acid, twenty of carbon, and eight of hydrogen; these constituents forming an acid equivalent in saturating power to one proportional of other acids. Hence it would seem, that half the sulphuric acid present, at least when in combination, is neutralized by the hydro-carbon; or, to speak in more general terms, that the hydro-carbon has diminished the saturating power of the sulphuric acid to one half. This very curious and interesting fact in chemical affinity was however made known to me by Mr. Hennell of Apothecaries' Hall, as occurring in some other compounds of sulphuric acid and hydro-carbon, before I had completed the analysis of the present acid and salts; and a similar circumstance is known with regard to muriatic acid, in the curious compound discovered by M. Kind, which it forms with oil of turpentine. Mr. Hennell is I believe on the point of offering an account of his experiments to the Royal Society, and as regards date they precede mine.

It may be observed, that the existence of sulphuric acid in the new compounds, is assumed, rather than proved; and that the non-appearance of sulphurous acid, when sulphuric acid and naphthaline act on each other, is not conclusive as to the non-reaction of the bodies. It is possible that part of the hydrogen of the naphthaline may take oxygen from one of the proportions of the sulphuric acid, leaving the hypo-sulphuric acid of Welter and Gay-Lussac, which with the hydro-carbon may constitute the new acid. I have not time at present to pursue these refinements of the subject, or to repeat the analyses which have been made of naphthaline, and which would throw light upon the question. Such a view would account for a part of the overplus in weight, but not for the excess of the sulphuric acid obtained, above two proportionals.

The glowing salt of baryta was now analysed by a process similar to that adopted for the flaming salt. The specimen operated upon was pure, and in a distinctly crystalline state. It had been heated to about 440° F. for three hours in a metallic bath. Ten grains of this salt exposed to air for forty hours increased only 0·08 of a grain in weight. These when converted into sulphate of baryta by heat and sulphuric acid, gave 4·24 grains. Seven grains by carbonate of baryta, oxide of copper, heat, &c. gave 6·02 grains of sulphate of baryta: hence 10 grains of the salt would have afforded 8·6 grains of the sulphate equivalent to 2·915 grains of sulphuric acid. Five grains when heated with oxide of copper gave 16·68 cubic inches of carbonic acid gas, equal to 7·772 grains, and equivalent to 2·12 grains of carbon. The water formed amounted to 1·2 grains, equivalent to 0·133 of a grain of hydrogen.

From these data, 100 grains of the salt would appear to furnish

Baryta	28·03	78	or 1 proportional.
Sulphuric acid	29·13	81·41	nearly 2 proportionals.
Carbon	42·40	118·	approaching to 20 ditto.
Hydrogen	2·66	7·4	or 7·4 proportionals.
	102·22		

results not far different from those obtained with the former salt.

I have not yet obtained sufficient quantities of this salt in a decidedly crystalline state to enable me satisfactorily to account for the difference between it and the flaming salt.

Attempts were made to form similar compounds with other acids than the sulphuric. Glacial phosphoric acid was heated and shaken in naphthaline, but without any particular results. A little water was then used with another portion of the materials, to bring the phosphoric acid into solution, but no decided combination could be obtained. Muriatic acid gas was brought into contact with naphthaline in various states, and at various temperatures, but no union could be effected either of the substances or their elements.

Very strong solution of potash was also heated with naphthaline, and then neutralized by sulphuric acid; nothing more however than common sulphate of potash resulted.

As the appropriation of a name to this acid will much facilitate future reference and description, I may perhaps be allowed to suggest that of *sulpho-naphthalic acid*, which sufficiently indicates its source and nature without the inconvenience of involving theoretical views.

Royal Institution, Jan. 10, 1826.

LX. *On*

LX. *On finding the Latitude, &c. from three Altitudes of the Sun and the elapsed Times.* *By* James Burns, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

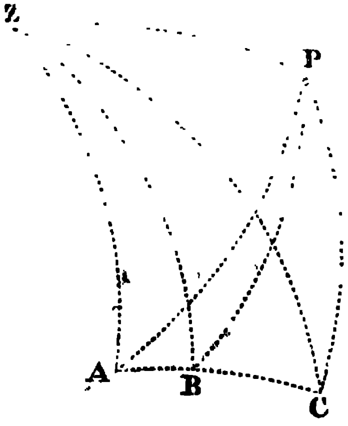
Sir,

I HESITATED some time whether I should again address you on any subject which may cause a controversy; but as I believe the following solution of another problem cannot be in that predicament, you are at liberty to make whatever use you please of it. You have already devoted more of your pages to a controversy (of which I have taken my leave) than its importance perhaps deserved; and therefore your last correspondent Mr. Beverley will excuse me, if I pass him in silence, merely reminding him:

> το γαρ περισσα πρασσειν ȣκ εχει νȣν ȣδενα.—*Soph. Antig.*

A controversy of that kind seldom ends in the conviction of either party, especially when there are misunderstandings on both sides; and I will readily own that my *first* method was not so free from objection as I considered it. But to come to our present purpose. *Three altitudes of the sun and the intervening intervals of time afford a ready and accurate method of determining the true time from noon, the sun's declination, and the latitude.* We have not however, so far as I have seen, an analytical solution of that problem, which can be considered short and practical*; or in other words, the formulæ of solution are not embodied in the smallest possible compass. Mackay in his very useful work on the longitude, has given us a geometrical solution, which is very ingenious, and which, I believe, was first given by D. Bernouilli. But the usual objection to most geometrical solutions may be made to this,—that it breaks the formulæ into too many equations, which render it more complicated. To the following solution, I believe this objection cannot be made.

Let $\quad ZA = a$
$ZB = b$
$ZC = c$
$ZP = \lambda$
$PC = PB = PA = \delta$
$ZPA = x$
$APB = y$
$APC = z$



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* Our correspondent will find an analytical solution of this problem, by Euler,

$$\text{Then, } \cos a = \cos\lambda \,.\, \cos\delta + \cos x \,.\, \sin\lambda \,.\, \sin\delta \;\ldots\ldots\; (1)$$

$$\cos b = \cos\lambda \,.\, \cos\delta + \cos(x+y) \,.\, \sin\lambda \,.\, \sin\delta \,..\, (2)$$

$$\cos c = \cos\lambda \,.\, \cos\delta + \cos(x+z) \,.\, \sin\lambda \,.\, \sin\delta \,..\, (3)$$

Subtracting (2) from (1),

$$\cos a - \cos b = \{\cos x - \cos(x+y)\} \,.\, \sin\lambda \,.\, \sin\delta \,\ldots\, (4)$$

Subtracting (3) from (1),

$$\cos a - \cos c = \{\cos x - \cos(x+z)\} \,.\, \sin\lambda \,.\, \sin\delta$$

$$\because \frac{\cos a - \cos b}{\cos a - \cos c} = \frac{\cos x - \cos(x+y)}{\cos x - \cos(x+z)} = \frac{\cos x - (\cos x \,.\, \cos y - \sin x \,.\, \sin y)}{\cos x - (\cos x \,.\, \cos z - \sin x \,.\, \sin z)}$$

$$= \frac{1 - \cos y + \tan x \,.\, \sin y}{1 - \cos z + \tan x \,.\, \sin z}.$$

Hence $\{(\cos a - \cos b) \,.\, \sin z - (\cos a - \cos c) \,.\, \sin y\} \tan x$ $= (\cos c - \cos a) \,.\, \cos y - (\cos b - \cos a) \,.\, \cos z + \cos b - \cos c$

$$\because \tan x = \frac{(\cos c - \cos a)\cos y - (\cos b - \cos a)\cos z + \cos b - \cos c}{-(\cos a - \cos c)\sin y + (\cos a - \cos b)\sin z} \quad (\alpha)$$

Now x is the time from noon of the observation nearest it; and y, z, the sum and one of the given intervals between the observations. Hence the true time is readily determined for either observation.

Again, we have from (4) $\sin\lambda \sin\delta = \dfrac{\cos a - \cos b}{\cos x - \cos(x+y)}$; and from (1), $\cos\lambda \cos\delta = \cos a - \sin\lambda \sin\delta \cos x$.

$$\text{Hence, } \cos\lambda \cos\delta = \cos a - \cos x \,.\left(\frac{\cos a - \cos b}{\cos x - \cos(x+y)}\right)$$

$$\because \sin\lambda \sin\delta + \cos\lambda \cos\delta$$

$$= \frac{\cos a - \cos b}{\cos x - \cos(x+y)} - \cos x \,.\left(\frac{\cos a - \cos b}{\cos x - \cos(x+y)}\right) + \cos a$$

$$\text{And, } \sin\lambda \sin\delta - \cos\lambda \cos\delta$$

$$= \frac{\cos a - \cos b}{\cos x - \cos(x+y)} + \cos x \,.\left(\frac{\cos a - \cos b}{\cos x - \cos(x+y)}\right) - \cos a$$

$$\text{Or, } \cos(\lambda - \delta) = (1 - \cos x) \,.\left\{\frac{\cos a - \cos b}{\cos x - \cos(x+y)}\right\} + \cos a \,..\, (\beta)$$

$$\text{And, } \cos(\lambda + \delta) = (1 + \cos x) \,.\left\{\frac{\cos a - \cos b}{\cos x - \cos(x+y)}\right\} - \cos a \,..\, (\gamma)$$

The following example will illustrate the use of these formulæ. Suppose at $9^h\ 51^m\ 58^s$ per watch, the sun's correct altitude was found 21° 11′; at $10^h\ 48^m\ 54^s$ it was 24° 40′, and at $11^h\ 29^m\ 42^s$ it was 26° 0′. Required the apparent time, when

Euler, at page 356 of the Gentleman's Mathematical Companion for the year 1815. We may also refer him to pages 624 and 71 of the same work for the years 1816 and 1821 respectively, for various solutions of two analogous problems.—Edit.

the observation nearest noon was taken, together with the latitude of the place and the sun's declination?

Here, $\cos a = 438371$
$\cos c = 361353$

77018 ... log. $4{\cdot}88659$
$\cos y$ $9{\cdot}99308$

-75800 log. ... $4{\cdot}87967$
$\cos a = 438371$
$\cos b = 417339$

21032 ... log. ... $4{\cdot}32288$
$\cos z$ ... $9{\cdot}95925$

$+\ 19148$ ... log. ... $4{\cdot}28213$

$\cos b = 417339$ log. 77018 ... $4{\cdot}88659$
$\cos c = 361353$ $\sin y$ ... $9{\cdot}24818$

$+\ \ 55986$ -13639 ... log. ... 4.13477

log. 21032 ... $4{\cdot}32288$
$\sin z$ ... $9{\cdot}61662$

$+8699$ ... log. ... $3{\cdot}93950$

The formula (α) therefore reduced to numbers will stand as follows:

$$\tan x = \frac{-75800+19148+55986}{-13639+8699} = \frac{-666}{-4940}$$

log. 666 ... $2{\cdot}82347$
log. 4940 ... $3{\cdot}69373$

tan $7^\circ\ 41' = {\cdot}13482$ log. ... $\bar{1}{\cdot}12974$

Now, $7^\circ\ 71' = 30^m\ 44^s$ in time; therefore the apparent time of the observation nearest noon was $11^h\ 29^m\ 16^s$.

Now to find the latitude and declination:

$1 - \cos x = 89{\cdot}779$ ... log. $1{\cdot}95317$
$\cos a - \cos b = 210{\cdot}326$ ... log. $2{\cdot}32288$

$4{\cdot}27605$
$\cos x - \cos (x + y)$ ... log. ... $2{\cdot}59481$

48 ... log. ... $1{\cdot}68124$
$\cos a$ ... 4384

$63^\circ\ 41'\tfrac{1}{2}$ cos. ... 4432

1 +

$1 + \cos x = 19910{\cdot}2$ ... log. 4·29907

$\cos a - \cos b = 210{\cdot}3$ ... log. 2·32288

6·62195

$\cos x - \cos (x + y)$ ... log. 2·59481

10645 ... log. 4·02714

$\cos a$ 4384

51° 14′ ... cos 6261

63 41½

2) 114 55½

57 27¾ = latitude.

2) 12 27½

6 13¾ = declination.

This declination answers to March 4th, or October the 9th.

In using formula (α), particular attention must be paid to the signs of the quantities. I am not aware that there is any other difficulty attending the use of it.

I remain, sir, your obedient servant,

James Burns.

Gloucester Place, Hackney Road,
March 23, 1826.

LXI. *Character and Description of* Kingia, *a new Genus of Plants found on the South-west Coast of New Holland: with Observations on the Structure of its Unimpregnated Ovulum; and on the female Flower of Cycadeæ and Coniferæ. By* Robert Brown, *Esq., F.R.S.S.L.& E., F.L.S.*

[Concluded from p. 361.]

THE account which I have given of the structure of the vegetable ovulum, differs essentially from all those now quoted, and I am not acquainted with any other observations of importance respecting it.

Of the authors referred to, it may be remarked, that those who have most particularly attended to the ovulum externally, have not always examined it at a sufficiently early period, and have confined themselves to its surface: that those who have most minutely examined its internal structure, have trusted too much to sections merely, and have neglected its appearance externally: and that those who have not at all examined it in the early stage, have given the most correct account of its surface. This account was founded on a very limited observation of ripe seeds, generalized and extended to the unimpregnated ovulum, in connexion with an hypothesis then very commonly received: but this hypothesis being soon after abandoned, their statement respecting the ovulum was rejected along with it.

In the ovulum of Kingia, the inner membrane, with relation to the external umbilicus, is inverted; and this, as I have already observed, though in direct opposition to M. Turpin's account, is the usual structure of the organ. There are, however, several families in each of the two primary divisions of phænogamous plants, in which the inner membrane, and consequently the nucleus, agrees in direction with the testa. In such cases the external umbilicus alone affords a certain indication of the position of the future embryo.

It is an obvious consequence of what has been already stated, that the radicle of the embryo can never point directly to the external umbilicus or hilum, though this is said to be generally the case by the most celebrated carpologists.

Another observation may be made, less obviously a consequence of the structure described, but equally at variance with many of the published accounts and figures of seeds; namely, that the radicle is never absolutely inclosed in the albumen; but, in the recent state, is either immediately in contact with the inner membrane of the seed, or this contact is established by means of a process generally very short, but sometimes of great length, and which indeed in all cases may be regarded as an elongation of its own substance. From this rule I have found one apparent deviation, but in a case altogether so peculiar, that it can hardly be considered as setting it aside.

It is necessary to observe, that I am acquainted with exceptions to the structure of the ovulum as I have here described it. In Compositæ its coats seem to be imperforated, and hardly separable, either from each other or from the nucleus. In this family, therefore, the direction of the embryo can only be judged of from the vessels of the testa*. And in Lemna I have found an apparent inversion of the embryo with relation to the apex of the nucleus. In this genus, however, such other peculiarities of structure and economy exist, that, paradoxical as the assertion may seem, I consider the exception rather as confirming than lessening the importance of the character.

It may perhaps be unnecessary to remark, that the raphe, or vascular cord of the outer coat, almost universally belongs to that side of the ovulum which is next the placenta. But it is at least deserving of notice, that the very few apparent exceptions to this rule evidently tend to confirm it. The most remarkable of these exceptions occur in those species of Euonymus, which, contrary to the usual structure of the genus and family they belong to, have pendulous ovula; and, as I have long since noticed, in the perfect ovula only of Abelia*. In these, and in the other cases in which the raphe is on the outer side, or that most remote from the placenta, the ovula are in reality resupinate; an economy apparently essential to their developement.

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* *Linn. Soc. Transact.* xii. p. 136.

The distinct origins and different directions of the nourishing vessels and channel through which fecundation took place in the ovulum, may still be seen in many of those ripe seeds that are winged, and either present their margins to the placenta, as in Proteaceæ, or have the plane of the wing at right angles to it, as in several Liliaceæ. These organs are visible also in some of those seeds that have their testa produced at both ends beyond the inner membrane, as Nepenthes; a structure which proves the outer coat of scobiform seeds, as they are called, to be really testa, and not arillus, as it has often been termed.

The importance of distinguishing between the membranes of the unimpregnated ovulum and those of the ripe seed must be sufficiently evident from what has been already stated. But this distinction has been necessarily neglected by two classes of observers. The first consisting of those, among whom are several of the most eminent carpologists, who have regarded the coats of the seed as products of fecundation. The second, of those authors who, professing to give an account of the ovulum itself, have made their observations chiefly, or entirely, on the ripe seed, the coats of which they must consequently have supposed to be formed before impregnation.

The consideration of the *arillus*, which is of rare occurrence, is never complete, and whose developement takes place chiefly after fecundation, might here, perhaps, be entirely omitted. It is, however, worthy of remark, that in the early stage of the ovulum, this envelope is in general hardly visible even in those cases where, as in Hibbertia volubilis, it attains the greatest size in the ripe seed; nor does it in any case, with which I am acquainted, cover the foramen of the testa until after fecundation.

The *testa*, or outer coat of the seed, is very generally formed by the outer membrane of the ovulum; and in most cases where the nucleus is inverted, which is the more usual structure, its origin may be satisfactorily determined; either by the hilum being more or less lateral, while the foramen is terminal; or more obviously, and with greater certainty where the *raphe* is visible, this vascular cord uniformly belonging to the outer membrane of the ovulum. The *chalaza*, properly so called, though merely the termination of the raphe, affords a less certain character, for in many plants it is hardly visible on the inner surface of the testa, but is intimately united with the areola of insertion of the inner membrane or of the nucleus, to one or other of which it then seems entirely to belong. In those cases where the testa agrees in direction with the nucleus, I am not acquainted with any character by which it can be absolutely distinguished from the inner membrane in the ripe seed; but as a few plants are already known, in which the outer membrane is originally incomplete, its entire absence, even before fecundation, is conceivable; and some possible cases of such a structure will be mentioned hereafter.

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* Abel's *China*, p. 377.

There are several cases known, some of which I have formerly noticed*, of the complete obliteration of the testa in the ripe seed; and on the other hand it appears to constitute the greater part of the substance of the bulb-like seeds of many Liliaceæ, where it no doubt performs also the function of albumen, from which, however, it is readily distinguished by its vascularity †. But the most remarkable deviation from the usual structure and economy of the outer membrane of the ovulum, both in its earliest stage and in the ripe fruit, that I have yet met with, occurs in Banksia and Dryandra. In these two genera I have ascertained that the inner membrane of the ovulum, before fecundation, is entirely exposed, the outer membrane being even then open its whole length; and that the outer membranes of the two collateral ovula, which are originally distinct, cohere in a more advanced stage by their corresponding surfaces, and together constitute the anomalous dissepiment of the capsule; the inner membrane of the ovulum consequently forming the outer coat of the seed.

The *inner membrane* of the ovulum, however, in general appears to be of greater importance as connected with fecundation, than as affording protection to the nucleus at a more advanced period. For in many cases, before impregnation, its perforated apex projects beyond the aperture of the testa, and in some plants puts on the appearance of an obtuse, or even dilated stigma; while in the ripe seed it is often either entirely obliterated, or exists only as a thin film, which might readily be mistaken for the epidermis of a third membrane then frequently observable.

This *third coat* is formed by the proper membrane or cuticle of the nucleus, from whose substance in the unimpregnated ovulum it is never, I believe, separable, and at that period is very rarely visible. In the ripe seed it is distinguishable from the inner membrane only by its apex, which is never perforated, is generally acute and more deeply coloured, or even sphacelated.

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* *Linn. Soc. Transact.* xii. p. 149.

† Ibid.

The membrane of the nucleus usually constitutes the innermost coat of the seed. But in a few plants an additional coat, apparently originating in the inner membrane of Grew, the vesicula colliquamenti or amnios of Malpighi also exists.

In general the amnios, after fecundation, gradually enlarges, till at length it displaces or absorbs the whole substance of the nucleus, containing in the ripe seed both the embryo and albumen, where the latter continues to exist. In such cases, however, its proper membrane is commonly obliterated, and its place supplied either by that of the nucleus, by the inner membrane of the ovulum, or, where both these are evanescent, by the testa itself.

In other cases the albumen is formed by a deposition of granular matter in the cells of the nucleus. In some of these cases the membrane of the amnios seems to be persistent, forming even in the ripe seed a proper coat for the embryo, the original attachment of whose radicle to the apex of this coat may also continue. This, at least, seems to me the most probable explanation of the structure of true Nymphæaceæ, namely, Nuphar, Nymphæa, Euryale, Hydropeltis, and Cabomba, notwithstanding their very remarkable germination, as observed and figured in Nymphæa and Nuphar, by Tittmann*.

In support of this explanation, which differs from all those yet given, I may here advert to an observation published many years ago, though it seems to have escaped every author who has since written on the subject, namely, that before the maturity of the seed in Nymphæaceæ, the sacculus contains along with the embryo a (pulpy or semi-fluid) substance, which I then called Vitellus, applying at that time this name to every body interposed between the albumen and embryo †. The opinion receives some confirmation also from the existence of an extremely fine filament, hitherto overlooked, which, originating from the centre of the lower surface of the sacculus, and passing through the hollow axis of the albumen, probably connects this coat of the embryo in an early stage with the base of the nucleus.

The same explanation of structure applies to the seeds of Piperaceæ and Saururus; and other instances occur of the persistence either of the membrane or of the substance of the amnios in the ripe seed.

It may be concluded from the whole account which I have given of the structure of the ovulum, that the more important changes consequent to real, or even to spurious fecundation, must take place within the nucleus: and that the albumen, properly so called, may be formed either by a deposition or secretion of granular matter in the utriculi of the amnios, or in those of the nucleus itself; or lastly, that two substances having these distinct origins, and very different textures, may co-exist in the ripe seed, as is probably the case in Scitamineæ.

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* *Keimung der Pflanzen*, p. 19. & 27. tab. 3. & 4.

† *Prodr. Flor. Nov. Holl.* i. p. 306.

On the subject of the ovulum, as contained in an ovarium, I shall at present make but one other remark, which forms a necessary introduction to the observations that follow.

On the Structure of the female Flower in CYCADEÆ *and* CONIFERÆ.

That the apex of the nucleus is the point of the ovulum where impregnation takes place, is at least highly probable, both from the constancy in the appearance of the embryo at that point, and from the very general inversion of the nucleus; for by this inversion its apex is brought nearly, or absolutely, into contact with that part of the parietes of the ovarium, by which the influence of the pollen may be supposed to be communicated. In several of those families of plants, however, in which the nucleus is not inverted, and the placentæ are polyspermous, as Cistineæ [*], it is difficult to comprehend in what manner this influence can reach its apex externally, except on the supposition, not hastily to be admitted, of an impregnating aura filling the cavity of the ovarium; or by the complete separation of the fecundating tubes from the placentæ, which, however, in such cases I have never been able to detect.

It would entirely remove the doubts that may exist respecting the point of impregnation, if cases could be produced where the ovarium was either altogether wanting, or so imperfectly formed, that the ovulum itself became directly exposed to the action of the pollen, or its fovilla; its apex, as well as the orifice of its immediate covering, being modified and developed to adapt them to this economy.

But such, I believe, is the real explanation of the structure of Cycadeæ, of Coniferæ, of Ephedra, and even of Gnetum, of which Thoa of Aublet is a species.

To this view the most formidable objection would be removed, were it admitted, in conformity with the preceding observations,

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[*] This structure of ovulum, indicated by that of the seed, as characterizing and defining the limits of Cistineæ, (namely, Cistus, Helianthemum, Hudsonia and Lechea,) I communicated to Dr. Hooker, by whom it is noticed in his *Flora Scotica*, (p. 284,) published in 1821; where, however, an observation is added respecting Gærtner's description of Cistus and Helianthemum, for which I am not accountable.

servations, that the apex of the nucleus, or supposed point of impregnation, has no organic connexion with the parietes of the ovarium. In support of it, also, as far as regards the direct action of the pollen on the ovulum, numerous instances of analogous economy in the animal kingdom may be adduced.

The similarity of the female flower in Cycadeæ and Coniferæ to the ovulum of other phænogamous plants, as I have described it, is indeed sufficiently obvious to render the opinion here advanced not altogether improbable. But the proof of its correctness must chiefly rest on a resemblance, in every essential point, being established, between the inner body in the supposed female flower in these tribes, and the nucleus of the ovulum in ordinary structures; not only in the early stage, but also in the whole series of changes consequent to fecundation. Now as far as I have yet examined, there is nearly a complete agreement in all these respects. I am not entirely satisfied, however, with the observations I have hitherto been able to make on a subject naturally difficult, and to which I have not till lately attended with my present view.

The facts most likely to be produced as arguments against this view of the structure of Coniferæ, are the unequal and apparently secreting surface of the apex of the supposed nucleus in most cases; its occasional projection beyond the orifice of the outer coat; its cohesion with that coat by a considerable portion of its surface, and the not unfrequent division of the orifice of the coat. Yet most of these peculiarities of structure might perhaps be adduced in support of the opinion advanced, being apparent adaptations to the supposed economy.

There is one fact that will hardly be brought forward as an objection, and which yet seems to me to present a difficulty, to this opinion; namely, the greater simplicity in Cycadeæ, and in the principal part of Coniferæ, of the supposed ovulum which consists of a nucleus and one coat only, compared with the organ as generally existing when inclosed in an ovarium. The want of uniformity in this respect may even be stated as another difficulty, for in some genera of Coniferæ the ovulum appears to be complete.

In Ephedra, indeed, where the nucleus is provided with two envelopes, the outer may, perhaps, be supposed rather analogous to the calyx, or involucrum of the male flower, than as belonging to the ovulum; but in Gnetum, where three envelopes exist, two of these may, with great probability, be regarded as coats of the nucleus; while in Podocarpus and Dacrydium, the outer cupula, as I formerly termed it*, may also, perhaps, be viewed as the testa of the ovulum. To this view, as far as relates to Dacrydium, the longitudinal fissure of the outer coat in the early stage, and its state in the ripe fruit, in which it forms only a partial covering, may be objected *. But these objections are, in a great measure, removed by the analogous structure already described in Banksia and Dryandra.

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* Flinders's Voy. vol. ii. p. 573.

The plurality of embryos sometimes occurring in Coniferæ, and which, in Cycadeæ, seems even to be the natural structure, may also, perhaps, be supposed to form an objection to the present opinion, though to me [illegible] appears rather an argument in its favour.

Upon the whole, the objections to which the [illegible] here taken of the structure of these two families is still liable [illegible] to me, as far as I am aware of them, much less important than those that may be brought against the other opinions that have been advanced, and still divide botanists on [illegible] subject.

According to the earliest of these opinions, [illegible] female flower of Cycadeæ and Coniferæ is a monospermous pistillum, having no proper floral envelope.

To this structure, however, Pinus itself was long considered by many botanists as presenting an exception.

Linnæus has expressed himself so obscurely in the natural character which he has given of this genus, that I find it difficult to determine what his opinion of its structure really was. I am inclined, however, to believe it to have been much nearer the truth than is generally supposed; judging of it from a comparison of his essential with his artificial generic character, and from an observation recorded in his *Prælectiones*, published by Giseke †.

But the first clear account that I have met with, of the real structure of Pinus, as far as regards the direction, or base and apex of the female flowers, is given, in 1767, by Trew, who describes them in the following manner: "Singula semina vel potius germina stigmati tanquam organo feminino gaudent ‡," and his figure of the female flower of the Larch, in which the stigmata project beyond the base of the scale, removes all doubt respecting his meaning.

In 1789, M. de Jussieu, in the character of his genus Abies ||, gives a similar account of structure, though somewhat less clearly as well as less decidedly expressed. In the observations that follow, he suggests, as not improbable, a very different view, founded on the supposed analogy with Araucaria, whose structure was then misunderstood; namely, that the

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[illegible] Flinders's Voy. loc. cit.

† *Prælect. in Ord. Nat.* p. 589.

‡ *Nov. Act. Acad. Nat. Curios.* iii. p. 453. tab. 13. fig. 23.

|| *Gen. Pl.* p. 414.

inner scale of the female amentum is a bilocular ovarium, of which the outer scale is the style. But this, according to Sir James Smith *, was also Linnæus's opinion; and it is the view adopted in Mr. Lambert's splendid monograph of the genus published in 1803.

In the same year in which Mr. Lambert's work appeared, Schkuhr † describes, and very distinctly figures, the female flower of Pinus, exactly as it was understood by Trew, whose opinion was probably unknown to him.

In 1807, a memoir on this subject, by Mr. Salisbury, was published ‡, in wh[illegible] account of structure is given, in no important particular different from that of Trew and Schkuhr, with who[illegible]ervations he appears to have been unacquainted.

M. [illegible] in 1809 §, held the same opinion both with respect to [illegible]d to the whole natural family. But in 1812, in conjunc[illegible] M. Schoubert ||, he proposed a very different [illegible]cture of Cycadeæ and Coniferæ, stating, that in thei[illegible]wers there is not only a minute cohering perianthium pre[illegible] but an external additional envelope, to which he has given the name of cupula.

In 1814 I adopted this view, as far, at least, as regards the manner of impregnation, and stated some facts in support of it ¶. But on re-considering the subject, in connexion with what I had ascertained respecting the vegetable ovulum, I soon after altogether abandoned this opinion, without, however, venturing explicitly to state that now advanced, and which had then suggested itself **.

It is well known that the late M. Richard had prepared a very valuable memoir on these two families of plants; and he appears, from some observations lately published by his son, M. Achille Richard ††, to have formed an opinion respecting their structure somewhat different from that of M. Mirbel, whose cupula is, according to him, the perianthium, more or less cohering with the included pistillum. He was probably led to this view, on ascertaining, which I had also done, that the common account of the structure of Ephedra was incorrect || ||, its supposed style being in reality the elongated tubular apex of a membranous envelope, and the included body being evidently analogous to that in other genera of Coniferæ.

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* Rees's *Cyclop. art.* Pinus. † *Botan. Hanb.* iii. p. 276. tab. 308.

‡ *Linn. Soc. Transact.* viii. p. 308.

§ *Ann. du Mus. d'Hist. Nat.* tom. xv. p. 473.

|| *Nouv. Bulletin des Sc.* tom. iii. pp. 73, 85, & 121.

¶ Flinders's Voy. ii. 572.

** Tuckey's *Congo*, p. 454. and *Linn. Soc. Transact.* vol. xiii. p. 213.

†† *Dict. Class. d'Hist. Nat.* tom. iv. p. 395. *et* tom. v. p. 216.

|| || *Ibid.* tom. vi. p. 208.

To the earliest of the opinions here quoted, that which considers the female flower of Coniferæ and Cycadeæ as a naked pistillum, there are two principal objections. The first of these arises from the perforation of the pistillum, and the exposure of that point of the ovulum where the embryo is formed to the direct action of the pollen; the second, from the too great simplicity of structure of the supposed ovulum, which, I have shown, accords better with that of the nucleus as existing in ordinary cases.

To the opinions of MM. Richard and Mirbel, the first objection does not apply; but the second requires such additional weight, as to render those opinions much less probable, it seems to me, than that which I have endeavoured to support.

In supposing the correctness of this opinion to be admitted, a question connected with it, and of some importance, would still remain, namely, whether in Cycadeæ and Coniferæ the ovula are produced on an ovarium of reduced functions and altered appearance, or on a rachis or receptacle. In other words, in employing the language of an hypothesis, which, with some alterations, I have elsewhere attempted to explain and defend, respecting the formation of the sexual organs in Phænogamous plants *, whether the ovula in these two families originate in a modified leaf, or proceed directly from the stem.

Were I to adopt the former supposition, or that best agreeing with the hypothesis in question, I should certainly apply it, in the first place, to Cycas, in which the female spadix bears so striking a resemblance to a partially altered frond or leaf, producing marginal ovula in one part, and in another being divided into segments, in some cases nearly resembling those of the ordinary frond.

But the analogy of the female spadix of Cycas to that of Zamia is sufficiently obvious; and from the spadix of Zamia to the fruit-bearing squama of Coniferæ, strictly so called, namely, of Agathis or Dammara, Cunninghamia, Pinus, and even Araucaria, the transition is not difficult. This view is applicable, though less manifestly, also to Cupressinæ; and might even be extended to Podocarpus and Dacrydium. But the structure of these two genera admits likewise of another explanation, to which I have already adverted.

If, however, the ovula in Cycadeæ and Coniferæ be really produced on the surface of an ovarium, it might, perhaps, though not necessarily, be expected that the male flowers should differ from those of all other phænogamous plants, and in this difference exhibit some analogy to the structure of the

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* *Linn. Soc. Transact.* vol. xiii. p. 211.

female flower. But in Cycadeæ, at least, and especially in Zamia, the resemblance between the male and female spadices is so great, that if the female be analogous to an ovarium, the partial male spadix must be considered as a single anthera, producing on its surface either naked grains of pollen, or pollen subdivided into masses, each furnished with its proper membrane.

Both these views may at present, perhaps, appear equally paradoxical; yet the former was entertained by Linnæus, who expresses himself on the subject in the following terms, "Pulvis floridus in Cycade minime pro Antheris agnoscendus est sed pro nudo polline, quod unusquisque qui unquam pollen antherarum in plantis examinavit fatebitur *." That this opinion, so confidently held by Linnæus, was never adopted by any other botanist, seems in part to have arisen from his having extended it to dorsiferous Ferns. Limited to Cycadeæ, however, it does not appear to me so very improbable, as to deserve to be rejected without examination. It receives, at least, some support from the separation, in several cases, especially in the American Zamiæ, of the grains into two distinct, and sometimes nearly marginal, masses, representing, as it may be supposed, the lobes of an anthera; and also from their approximation in definite numbers, generally in fours, analogous to the quaternary union of the grains of pollen, not unfrequent in the antheræ of several other families of plants. The great size of the supposed grains of pollen, with the thickening and regular bursting of their membrane, may be said to be circumstances obviously connected with their production and persistence on the surface of an anthera, distant from the female flower; and with this economy, a corresponding enlargement of the contained particles or fovilla might also be expected. On examining these particles, however, I find them not only equal in size to the grains of pollen of many antheræ, but, being elliptical and marked on one side with a longitudinal furrow, they have that form which is one of the most common in the simple pollen of phænogamous plants. To suppose, therefore, merely on the grounds already stated, that these particles are analogous to the fovilla, and the containing organs to the grains of pollen in antheræ of the usual structure, would be entirely gratuitous. It is, at the same time, deserving of remark, that were this view adopted on more satisfactory grounds, a corresponding developement might then be said to exist in the essential parts of the male and female organs. The increased developement in the ovulum would not consist so much in the unusual form and thickening of the

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* *Mém. de l'Acad. des Scien. de Paris,* 1775, p. 518.

coat, a part of secondary importance, and whose nature is disputed, as in the state of the nucleus of the seed, respecting which there is no difference of opinion; and where the plurality of embryos, or at least the existence and regular arrangement of the cells in which they are formed, is the uniform structure in the family.

The second view suggested, in which the anthera in Cycadeæ is considered as producing on its surface an indefinite number of pollen masses, each enclosed in its proper membrane, would derive its only support from a few remote analogies: as from those antheræ, whose loculi are sub-divided into a definite, or more rarely an indefinite, number of cells, and especially from the structure of the stamina of Viscum album.

I may remark, that the opinion of M. Richard *, who considers these grains, or masses, as unilocular antheræ, each of which constitutes a male flower, seems to be attended with nearly equal difficulties.

The analogy between the male and female organs in Coniferæ, the existence of an open ovarium being assumed, is at first sight more apparent than in Cycadeæ. In Coniferæ, however, the pollen is certainly not naked, but is enclosed in a membrane similar to the lobe of an ordinary anthera. And in those genera in which each squama of the amentum produces two marginal lobes only, as Pinus, Podocarpus, Dacrydium, Salisburia, and Phyllocladus, it nearly resembles the more general form of the antheræ in other Phænogamous plants. But the difficulty occurs in those genera which have an increased number of lobes on each squama, as Agathis and Araucaria, where their number is considerable and apparently indefinite, and more particularly still in Cunninghamia, or Belis †, in which the lobes, though only three in number, agree in this respect, as well as in insertion and direction, with the ovula. The supposition, that in such cases all the lobes of each squama are cells of one and the same anthera, receives but little support either from the origin and arrangement of the

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* *Dict. Class. d'Hist. Nat.* tom. v. p. 216.

† In communicating specimens of this plant to the late M. Richard, for his intended monograph of Coniferæ, I added some remarks on its structure, agreeing with those here made. I at the same time requested that, if he objected to Mr. Salisbury's Belis as liable to be confounded with Bellis, the genus might be named Cunninghamia, to commemorate the merits of *Mr. James Cunningham*, an excellent observer in his time, by whom this plant was discovered; and in honour of *Mr. Allan Cunningham*, the very deserving botanist who accompanied Mr. Oxley in his first expedition into the interior of New South Wales, and Captain King in all his voyages of survey of the Coasts of New Holland.

the lobes themselves, or from the structure of other phænogamous plants: the only cases of apparent, though doubtful, analogy that I can at present recollect occurring in Aphyteia, and perhaps in some Cucurbitaceæ.

That part of my subject, therefore, which relates to the analogy between the male and female flowers in Cycadeæ and Coniferæ, I consider the least satisfactory, both in regard to the immediate question of the existence of an anomalous ovarium in these families, and to the hypothesis repeatedly referred to, of the origin of the sexual organs of all phænogamous plants.

In concluding this digression, I have to express my regret that it should have so far exceeded the limits proper for its introduction into the present work. In giving an account, however, of the genus of plants to which it is annexed, I had to describe a structure, of whose nature and importance it was necessary I should show myself aware; and circumstances have occurred while I was engaged in preparing this account, which determined me to enter much more fully into the subject than I had originally intended.

LXII. *Hydrographical Notices:—Remarks on the Method of investigating the Direction and Force of the Currents of the Ocean; Presence of the Water of the Gulf-Stream on the Coasts of Europe in January* 1822; *Summary of the Currents experienced by His Majesty's Ship Pheasant, in a Voyage from Sierra Leone to Bahia, and thence to New York; Stream of the River Amazons crossed, three hundred Miles from the Mouth of the River. By Capt.* Edward Sabine, *R.A. F.R. & L.S. &c.*

[Concluded from p. 339.]

THE following summary account of the direction and force of the currents experienced in each day's navigation, commences with the appointment of the Pheasant to convey the clocks and pendulums from Sierra Leone to the subsequent stations. Captain Clavering entered with much interest into the inquiry, and by his judicious arrangements and personal superintendence, until habits were established, the reckoning of his ship was rendered little inferior, as an element in the deduction of currents, to the observed difference of latitude and the chronometrical difference of longitude. On leaving England, I had obtained from the Admiralty a supply of the logs invented by Mr. Massey, which being towed at a sufficient distance astern, to be cleared of the back-water occasioned by a ship's progress, registered her way by the revolutions of a spiral acted upon by the water through which it is drawn. The self-registering log was used as a check upon the estimated reckoning, and proved the value and efficacy of the attention paid to the latter, by its being a very rare circumstance to find a difference between them amounting to a mile, in twenty-four hours. The comparison between the ship's run by observation and by reckoning was usually made by Capt. Clavering from forenoon to forenoon, and by myself from afternoon to afternoon; and the results being each reduced to noon and compared, served for the detection and correction of errors, on either side. The table exhibits the ship's true position at noon on each day; the temperature of the surface water; and the direction and amount of the difference of her position, by observation and by reckoning, from noon to noon. On days when the sun was obscured, the direction of the apparent set is deduced from intervals of forty-eight hours instead of twenty-four, but the rate is that due to each interval of twenty-four hours.

Date.	Latitude.	Longitude.	Temp. of the Surface Water.	Apparent Set in each 24 hours.
1822.	From Cape Mount to Cape Three Points.			
April 15	6° 40′ N.	11° 48′ W.	84°	S. 53° E. 32 miles.
16	Sun obscured.		83	
17	4 53	9 04	83	S. 84 E. 24
18	4 28	8 18	...	N. 79 E. 40
19	4 18	6 36	84·8	N. 76 E. 51
20	4 37	3 48	84·5	
	From Lagos to St. Thomas.			
May 8	5° 22′ N.	2° 51′ E.	83°·5	S. 45° E. 9 miles.
9	5 00	2 32	84·5	S. 84 E. 17
10	4 46	2 49	84	S. 24 E. 16
11	3 46	2 57	83·2	S. 82 E. 22
12	Sun obscured.		83	
13	0 36	5 22	82·8	S. 81 E. 13
14	0 16	6 24	82·8	

Date.

Date.	Latitude.	Longitude.	Temp. of the Surface Water.	Apparent Set in each 24 hours.
	From the river Gaboon to Ascension.			
1822. June 15	0° 03′ N.	7° 45′ E.	. .°	S. 80° W. 29 miles.
16	0 44 S.	5 50	. .	West 48·5
17	Sun obscured.		73	
18	1 00	2 07	74	S. 86 W. 29
19	1 45	0 19	72·5	N. 88 W. 37
20	2 34	1 55 W.	72·8	S. 81 W. 47
21	3 48	4 54	74·5	S. 81 W. 32·5
22	5 10	7 50	77·5	N. 63 W. 16
23	6 21	10 43	77·5	N. 57 W. 18·25
24	7 27	13 22	78	
	From Ascension to Bahia.			
July 10	7° 57′ S.	14° 24′ W.	. .	N. 74° W. 11 miles.
11	9 16	17 00	. .	North 2
12	10 10	19 45	. .	N. 35 W. 6
13	10 35·5	22 25	. .	West 16
14	11 05	25 53	. .	S. 82 W. 14
15	11 42	29 08	. .	S. 71 W. 14
16	12 27	32 51	. .	N. 79 W. 11
17	13 05	36 31	. .	
	From Bahia to Pernambuco.			
Aug. 8	13° 30′ S.	38° 22′ W.	. .°	N. 69 W. 13 miles.
9	Sun obscured.		77·2	
10	13 48	37 59	77·1	N. 12 W. 2·5
11	12 36·5	37 02	77·2	N. 31 W. 14
12	11 03·5	36 20	78	N. 33 E. 13
13	10 15	35 53·5	78	N. 27 W. 15
14	9 33	35 13	78	

Date.	Latitude.	Longitude.	Temp. of the Surface Water.	Apparent Set in each 24 hours.
	From Pernambuco to Maranham.			
1822.				
Aug. 15	8° 04′ S.	34° 54′ W.	78°	North 22 miles.
16	6 15	34 36	78·4	N. 44 W. 62
17	3 22	36 45	78·3	N. 70 W. 41
18	2 17·5	40 17	77·8	N. 66 W. 13
19	1 55	[illegible] 06	77·8	
	[illegible] Maranham to Trinidad.			
Sept. 8	0° 21′ N.	[illegible]′ W.	79°·8	N. 49 W. 48 miles.
9	2 59	[illegible]	80·8	N. 54 W. 99
10	5 1[illegible]	[illegible]	81·8	N. 38 W. 68
11	7 01	[illegible] 33	81·5	N. 41 E. 5
12	7 0[illegible]	[illegible] 32	83	S. 47 W. 18
13	7 [illegible]	[illegible] 19	83·3	S. 87 W. 17
14	7 43	[illegible] 55	84	N. 72 W. 28
15	8 1[illegible]	[illegible]	84	N. 33 W. 48
16	9 [illegible]	59 30	84	N. 52 W. 57
17	8 00	61 00	84	
	From Trinidad to Jamaica.			
Oct. 10	10° 55′	61° 56′	. .°	N. 52 W. 49 miles
11	12 24	63 43	83	N. 53 W. 12
12	13 18	65 56	83	N. 79 W. 16
13	13 53	67 59	82·8	S. 83 W. 16
14	15 02	70 45	82·9	N. 41 W. 19
15	Sun obscured.		83	
16	17 50	76 08	83	

Date.	Latitude.	Longitude.	Temp. of the Surface Water.	Apparent Set in each 24 hours.
1822.	From Havannah to New York.			
Nov. 27	23° 09′	82° 23′	. . [illegible]	S. 85 E. 14 miles.
28	23 52	81 42	. . 80·5	N. 31 E. 22·5
29	25 20	79 47	. . 80·7	
			8 A.M. 80·8	N. 4 W. 70
[illegible]	28 38	79 32	Noon [illegible]	
			9 P.M. [illegible]	N. 17 E. 38
			8 A.M. [illegible]	
Dec. 1	32 02	78 33	3 P.M. [illegible]	
			8 P.M. [illegible]	
			8 A.M. 78·2	
2	Sun obscured.		Noon 78[illegible]	N. [illegible] E. 44·5
			3 P.M. 7[illegible]	
			8 A.M. [illegible]	
3	35 04	74 54	Noon [illegible]	
			6 P.M. 77·3	
			8 A.M. 77·5	N. 55 E. 77
4	Sun obscured.		Noon 77·5	
5	36 38	72 29	. . 62·4	West 16
6	37 00	73 46	. . 60·6	
7	37 35	74 33	. . 59·5	S. 55 W. 10
8	38 44	74 26	. . [illegible]	S. 5 W. 15
9	40 08	74 07	. .	S. 45 W. 6

Remarks on the preceding Summary.

In the voyage between Cape Mount and Cape Three Points, the Pheasant's progress appears to have been accelerated about 180 miles, by the current, which during the season when the S.W. winds prevail on that part of the coast of Western Africa, runs with considerable rapidity in the direction of the land, round Cape Palmas to the eastern parts of the Gulf of Guinea. The breadth of this current abreast of Cape Palmas varies with the season, and has been found as much as 180 miles; but, in its subsequent course to the eastward, it enlarges to nearly 300, and occupies the whole space between the land on one side, and the equatorial current running in an opposite direction on the other; the velocity abreast of Cape Palmas and Cape Three Points, and in the vicinity of the land, was in May, about two knots an hour; and further to the eastward, where the Pheasant crossed its breadth from Cape Formosa to St. Thomas, and where its velocity had been much diminished by the dissipation of its waters, it was found still to preserve a general rate of rather less than a mile an hour; and a direction, a few degrees to the southward of east. Between Cape Three Points and Lagos, the observations were suspended in consequence of the greater part of the officers and men being absent in the boats, examining merchant-vessels anchored on the coast, and suspected of being engaged in the trade in slaves. The little effect of the current experienced between the 8th and 9th of May, was occasioned by the slack water in the Lagos bight, from which the Pheasant did not re-enter the fair stream until the morning of the 9th. There appears to have been a southerly deflection between the 10th and 11th, for which no very obvious reason presents itself. The general temperature of the stream in the mid-channel in the Gulf of Guinea, in April and May, exceeds 84 degrees, diminishing to 82 and 83 on its southern border, where it is in contact with the colder water of the equatorial current; and occasionally to 79°, and frequently to between 79° and 81°·5, on its northern side, in the proximity of land.

In the passage from the coast of Africa to the Island of Ascension, the Pheasant appears to have entered the equatorial current, almost immediately after her departure from the entrance of the River Gaboon; as she was decidedly under its influence when passing the southern extremity of the island of St. Thomas. This current is formed by the drift-water impelled by the trade-winds in the southern Atlantic (which in the neighbourhood of the continent of Africa are very much southwardly) towards the eastern part or head of the Gulf of Guinea; where, being opposed by the waters brought to the same spot by the Guinea current, the drift-water streams off in the direction of the equator, and principally on its southern side; and being continually fed in its western progress by the drift from the S.E. (becoming more and more inclined to the meridian, as the influence of the continent on the regular direction of the trade-wind lessens from distance) the stream pursues its course quite across the Atlantic to the continent of South America, where one portion of it proceeds along the northern coast of Brazil to the Caribbean Sea and Gulf of Mexico, and contributes in part to raise the level of those seas, and thus to lay the foundation of the Gulf-stream.

The Pheasant's voyages from the coast of Africa, successively to Ascension, Bahia, Pernambuco, Maranham, Trinidad, and Jamaica, were performed principally in the current, the origin and progress of which have been thus stated.

The equatorial current is not usually met with so far to the northward, at its commencement on the coast of Africa, as it was found by the Pheasant in the month of June: but it is probable that at the season when the trade-winds are strongest, and approach nearest the equator, the drift-water may be impelled into a more northern parallel than at other seasons, before the opposition to its direct course becomes so strong as to occasion it to stream off to the westward. Its more usual northern limit, in the meridian of the Island of St. Thomas, is considered by Major Rennell to be in the second or third degree of south latitude The direction of the stream was as nearly west as could be inferred from the observations, and its rapidity between the meridians of 7½ east, and 7½ west, averaged forty miles a-day. We appear to have passed out of the stream on the 22d of June in latitude 5° +, S., and longitude 8° +, W., into the drift current from the S.E., which contributes to its supply and to preserve its velocity across the Atlantic; it may be seen that the drift-water was pressing on the southern border of the stream with a force of 16 and 18 miles in 24 hours, in a direction oblique to and accelerative of its course.

In the passage between the River Gaboon & Ascension, being a distance of 1400 geographical miles, the Pheasant was aided by the current above 300 miles, in the direction of her course.

In consequence of the southing of the trade-wind in the vicinity of the continent of Africa, the water impelled before it, which forms the commencement of the Equatorial stream, arrives from a more remote southern parallel, and is therefore of a colder temperature than the drift-water which successively falls into it from the S.E., impelled more obliquely to the meridian, and consequently arriving from latitudes less distant from the equator. Thus the temperature of the stream varied from 72°·5 to 74°, whilst that of the drift-current was 77°·5 and 78°. But the more important distinction, both in amount and in utility in navigation, is between the waters of the Equatorial and of the Guinea currents. These exhibit the remark-

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able phænomenon of parallel streams, in contact with each other, flowing with great velocity, in opposite directions, and having a difference of temperature amounting to 10 and 12 degrees. Their courses continue thus parallel to each other and to the land for above a thousand miles; and according as a vessel, wishing to proceed along the coast in either direction, is placed in one or the other current, will her progress be aided from forty to fifty miles a-day, or retarded to the same amount: the practical advantage, therefore, derivable from the difference of temperature, in enabling vessels to discriminate at all times in which current they are situated, is as great as it is obvious*.

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* The occasional advance of the cold water of the Equatorial current to the island of St. Thomas, may assist in explaining an apparent peculiarity in the climate of that island, when compared with the climate of the coast of Western Africa generally. At all the British possessions, from the Gambia in 13° north latitude to the forts on the Gold Coast, June, July and August, are accounted the unhealthy months; whilst at St. Thomas, on the contrary, they are the most healthy in the year to Europeans, although they are not so to the Negroes, who suffer much from colds and rheumatism during their continuance. It has been seen, that the water of the Equatorial current is from 10 to 12 degrees colder than that of the Gulf of Guinea, and that its northern border, which at other seasons passes the meridian of St. Thomas at a distance from 120 to 180 miles south of its southern extremity, was found in June in contact, or very nearly so, with the island itself; and it is not improbable, from a consideration of the causes which occasion its advance towards the equator when the sun is in the northern signs, that in July it may extend so far, as even to include the whole island within its limits.

The temperature of the air is known to be immediately dependent on that of the surface-water of the sea, and to be influenced nearly to the full extent of any alteration that may take place therein. In crossing the Gulf of Guinea from Cape Formosa to St. Thomas, the air over the surface of the Guinea current, observed in the shade and to windward, at sunrise, noon, and sunset, averaged 81°·5, the extremes being 79° and 83°·5; whilst in the passage from the river Gaboon to Ascension, over the Equatorial current, the air averaged only 74°, the extremes being from 73°·5 to 74°·5; a part of the passage being, moreover, on the very edge of the two currents, and within sight of St. Thomas. The vicinity of the Equatorial current, therefore, when the sun is in the northern signs, cannot fail materially to influence the temperature of the island (particularly as the wind is always from the south), and thus to affect its climate. Situated on the equator, St. Thomas has naturally two cold seasons, or winters, in the year, the sun being equally distant in June and in December; but in June, July and August, is superadded the influence of the surface water of the ocean several degrees colder than in November, December, and January; rendering the months of June, July and August, pre-eminently the winter of St. *Thomas; in which the natives complain of colds and rheumatism, and the health of Europeans is less affected than at other seasons, because the* climate is then less dissimilar than usual to their own.

The comparative unhealthiness of Prince's Island to St. Thomas, and of both to Annabona, as the residence of Europeans, has been frequently and *particularly noticed by Portuguese authorities, and is universally recognized at Prince's*

The voyage from Ascension to Bahia commenced in the continuation of the same drift-current from the S.E. in which the latter part of the passage to Ascension was performed; but on the 13th of July, the Pheasant appears to have re-entered the southern border of the Equatorial current, in the longitude of 22½ W., and latitude of 10½ S. The evidence of many voyages in different years, the journals of which have been submitted to Major Rennell's examination, have led him to the conclusion, that it is the ordinary course of that stream, to divide into two branches about the twenty-third degree of west longitude: the northern portion flowing in a north-west direction, and diffusing its waters in the basin of the Atlantic; and the southern, which is the largest portion, proceeding in a direction to the southward of west, until it reaches the coast

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Prince's Island and at St. Thomas. It may be a sufficient explanation to remark, that Annabona is always surrounded by the Equatorial current; *Prince'* Island always by the Guinea current; and that the position of St. Thoma is intermediate, and its climate is occasionally influenced by both. In tropical climates a very few degrees of temperature constitute an essentia difference in the feelings of the natives, and in the health of Europeans.

The point of deposition varied over the differently-heated surfaces of water, in correspondence with the difference in the temperature of the air; so that, although the quantity of moisture was diminished in the colder air over the Equatorial current, the proportion of the quantity to that which would have been required for repletion, was as nearly as possible the same as over the Guinea current, being on the average 84°·5 parts in 100° in both instances. The air, therefore, was equally moist over the Equatorial as over the Guinea current, although in the one case the weight of vapour in a cubic foot (derived from the averages) was 10 grains, and in the other 7·93 grains only. The cold air incumbent on the Equatorial stream, being borne by the south wind over the surface of the Guinea current, caused the deposition, which generally obscured the horizon to the north of St. Thomas, during the pendulum observations; and which fell, as we understood, in heavy rain in the offing. The quantity of vapour in the atmosphere over the island being less than that over the nearly surrounding water of the Guinea current (an effect of the high land of which the island consists), no deposition took place on the island itself. The hygrometer indicated the temperature of its superincumbent vapour to be between the extremes of 71° and 74°·5, observed three times a-day between the 26th of May and the 12th of June. The range in the Gulf of Guinea was from 76° to 80°.

It is worthy of notice to what little distance the colder air, impelled by the constant south wind, attained over the Guinea current before it became itself heated by the condensation of the vapour of higher constituent temperature. The great bodies of the air and of the vapour over the respective currents, though so dissimilar in temperature, were as little affected by their contiguity, as the surface waters of the currents themselves. By their mutual and opposite action, the air in condensing and thus reducing the temperature of the vapour, and the heat liberated in the condensation of the vapour in raising that of the air, the mixture speedily destroyed the differences; and the effects of the contiguity were thus limited to a very few miles within the border of either stream.

of the continent of South America; where it is again subdivided by the projecting part of the coast between Cape St. Roque and Cape St. Augustin, the northern branch coasting the north of Brazil and Guiana to the West Indies, and the southern branch proceeding down the eastern side of the continent towards Terra del Fuego. The Pheasant's experience corresponded in all respects with this general view. The direction of the southern part of the Equatorial stream, into which she entered on the 13th of July, became gradually more and more to the southward of west on approaching the continent; being due west between the longitudes of 22°½ and 26°; S. 82 W. between 26° and 29°; and S. 71 W. between 29° and 33°; and the apparent set between the noons of the 16th and 17th of July is obviously compounded of the influence of the Equatorial stream (then probably become still more southwardly) during the first part of the twenty-four hours, and of the northerly current, during the latter part, which the observations between Bahia and Pernambuco show to prevail in the vicinity of the coast included between those stations. The Pheasant may, therefore, be considered to have crossed the whole breadth of the branch of the stream which proceeds to the S.W., by having passed out on its western side between the longitudes of 33° and 36°, and to have ascertained its general velocity to have exceeded half a mile an hour, by the according observations of the 14th, 15th, and 16th of July.

From Pernambuco to Cape St. Roque, the northerly current rapidly accelerated, until in passing the Cape it may be considered that the Pheasant had entered the full stream of the other branch of the Equatorial current; namely, of the one which pursues its way along the northern coast of Brazil and Guiana to the West Indies.

Between the noons of the 16th and 17th, she was set 44·5 to the north, and 42·5 to the west, making a general effect in the twenty-four hours of N. 44 W., 62 miles; but as she did not round Cape St. Roque until midnight, the course having been altered for that purpose at half-past eleven P.M., it must be understood that the direction of the current was probably more northerly in the first part of the interval, and more westerly in the latter part, than the general effect; and that the velocity may in like manner have been less than the rate of 62 miles to the south of Cape St. Roque, and more than that amount after passing the Cape. The purpose of stopping at Maranham, obliged the Pheasant to draw nearer the land on the following day, than would have been expedient, had she been bound direct to the West Indies, and been desirous of preserving the full advantage of the current in her favour; on examination of the tabular results, it will be obvious, that by thus nearing the land, she quitted the full strength of the stream, and that she did not re-enter it again until the day after her departure from Maranham, when it was found to be running with the astonishing rapidity of ninety-nine miles in twenty-four hours. It may also be seen, that although in the space comprised between the direct course of the stream from Cape St. Roque to the West Indies, and the coast of Brazil, the velocity progressively diminished on approaching the land, no counter-current was found to take place, but the westerly direction was still maintained, though at the reduced rate of less than half a mile an hour, when very near the land. It may be attributed to the rapidity with which the water is thus swept along the shore, that no change is perceptible in its temperature, on approaching a coast which is so remarkably shallow, as to have not more than seventeen fathoms water at thirty-six miles in the offing.

At 10 A.M. on the 10th of September, whilst proceeding in the full strength of the current, exceeding as already noticed four knots an hour, a sudden and very great discoloration in the surface water a-head was reported from the mast-head, and from the very rapid progress which the ship was making was almost immediately afterwards visible from the deck. Her position in 5° 08′ north latitude, and 50° 28′ west longitude, both known by observation, sufficiently apprised us that the discoloured water which we were approaching could be no other than the stream of the river Amazon, preserving its original impulse at a distance of not less than 300 miles from the mouth of the river, and its waters being not yet wholly mingled with those of the ocean of greater specific gravity over the surface of which it had pursued its course.

We had just time to secure some of the blue water of the ocean for subsequent examination, and to ascertain its temperature, before we crossed the line of its separation from the river-water, the division being as distinctly preserved as if they had been different fluids.

The direction of the line of separation was N.W. by N., rather northerly; great numbers of gelatinous marine animals, species of the genus *Physalia*, were floating on the edge of the river-water, and many birds were fishing apparently on both sides of the boundary.

The temperature of the ocean-water was 81°·1, and of the river-water 81°·8, both within a short distance of the division line; the specific gravity of the former was 1·0262, and of the latter 1·0204, distilled water being unity: the ocean-water had also been found 81° at 7 A.M. on the same morning.

At noon, having advanced considerably within the boundary, so that it was no longer in sight from the ship, the specific gravity of the surface water was 1·0185, and its temperature 81°·8.

Being desirous of ascertaining the depth at which the water of the ocean would be found unmixed with the river-water, Dr. Marcet's very simple and practical apparatus was employed to bring up water from fifty fathoms, the specific gravity of which proved 1·0262; the bottle was then sent down a second time to twenty-one fathoms, at which depth the specific gravity was also 1·0262, limiting [illegible] depth of any admixture of the fresh water to less than 126 feet. Its superficiality was further evinced by the colour of the water in the ship's wake, which was much more blue than that of the general surface. The temperature of the water from fifty fathoms was 77°·2, and from twenty-one fathoms, 80°·5; we had no bottom with 105 fathoms.

From noon on the 9th, till 10 A.M. on the 10th, we had found the current of the ocean running with an average velocity of four knots in a direction N. 54° W.; the ship's true course had been very nearly N. 45° W.; the division line of the streams trended about N. 33° W. It was obvious, by the general appearance of the respective surfaces, that the current of the river-water was running with considerable rapidity in a direction inclined to that of the ocean, and nearly coinciding with the line which marked their separation; the ship's course was therefore altered a point westerly. During the afternoon of the 10th, and morning of the 11th, the colour and specific gravity of the surface-water indicated that we continued in the river-stream; but that it was becoming latterly more and more mixed with the sea-water. At noon, in latitude 7° 01′, and longitude 52° 38′·5, the specific gravity was 1·0248, temperature 81°·5; and from twenty fathoms, 1·0262. Between noon on the 10th, and noon on the 11th, the ship was set N. 38° W., sixty-eight miles, or rather less than three miles an hour; which may, therefore, be considered the general direction and rate at which the water of the Amazon was proceeding at the distance of 300 miles and upwards from its natural banks. The original impulse at its discharge into the ocean is to the eastward of north; so much, therefore, had its course been deflected, by having to sustain the continual pressure of the current of the ocean on its eastern side. As the initial velocity must have greatly exceeded that which it had preserved after a course of 300 miles, and as the force of the current which presses on it is much less in the neighbourhood of the land, than it subsequently becomes, it is probable that the deflection may have been scarcely sensible in the early part of the course, but much more rapid latterly than would be due to the whole effect divided by the distance; and that a further deflection of the 16 degrees, which measured the inclination of the streams where the Pheasant crossed the division-line, might not require much more distance for its accomplishment; when the course of the streams being parallel, the obstacle to the diffusion of the river-water on its eastern side would [illegible], and the marked line of the separation of the [illegible] would gradually cease to exist. In the early part of the river's marine course, as it may be termed, and where [illegible] force of the current of the ocean is comparatively weak, the greater obliquity of its direction may compensate for its want of force, in enabling it to oppose the diffusion of the river-water. On the western side the fresh water is gradually and insensibly lost in that of the sea; at noon on the 12th, the specific gravity of the surface-water was 1·0253, in latitude 7° 05′, and longitude 53°½.

The effect which the stream of the Amazons produces on the current of the ocean in thus crossing its course, is to accumulate the water brought by the Equatorial current, until it streams off with a rapidity which gradually deflects, and ultimately overpowers the obstacle, which opposes its more regular flow; it is to the accumulation from this cause, that the partial velocity of ninety-nine miles in twenty-four hours, much exceeding the average rate of the branch of the Equatorial current between Cape St. Roque and the West Indies, is to be attributed. The southern border of the current is also removed by it to a distance from the land, leaving a space of the ocean, bounded by the river-water on the east, the land on the south, and the Equatorial current on the north, which is occupied by irregular streams of various and uncertain strength and direction, as shown by the Pheasant's experience between the 11th and the 14th of September. It is desirable that vessels bound from the Brazils to the West Indies should, therefore, keep well off the land of Guiana, in order to preserve the strength of the Equatorial current in their favour; whilst others, endeavouring to make a passage along the coast to the eastward, should be especially cautious to keep in the space within the current. The Pheasant re-entered the current about the eighth degree of latitude, and in the fifty-seventh of longitude, and was subsequently indebted to its influence, between two miles and two miles and a half an hour, until her arrival in the Gulf of Paria*.

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* In the passage from Maranham to the West Indies, and in crossing the mouth of one of the largest rivers of the globe, the hygrometrical state of

The observations in the passage from Trinidad to Jamaica, indicate a general set of the surface-water across the Caribbean sea towards the Gulf of Mexico, averaging about sixteen miles in each twenty-four hours. The northerly inflexion, on approaching Jamaica, was occasioned by the indraft between Cape Tiburon and Point Morant.

From Jamaica to the Havannah the Pheasant was engaged in conducting a convoy, which obliged a suspension of the observations.

In crossing the Caribbean sea from Trinidad to Jamaica, between the 9th and 17th of October, the temperature of the surface-water, observed always at 8 A.M., and occasionally at other hours, was never less than 82°·8, nor more than 83°; between Jamaica and Grand Cayman, on the 10th and 11th of November, the minimum was 83°, and the maximum at 3 P.M., 83°·8; from the Cayman Islands to the entrance into the Gulf of Mexico, between Yucatan and Cuba, and in the open part of the Gulf itself, the surface varied from 82° to 82°·5*; but on

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of the atmosphere was the subject of very frequent and careful observation on each day; no effect of the river, however, on the state of the aqueous vapour was perceptible; the point of deposition varied only between 72°·5 and 74°, and the air between 79° and 82°, the higher temperatures of both taking place when we had arrived abreast of Surinam, and the surface water had increased to 83°. In the Gulf of Paria, where the general temperature of the surface is raised to 84°·5, by the admixture of the heated water from the smaller branches of the Orinoco, the air was further augmented to 84°, and the point of deposition to 75°·5 Between Point Galeotta and Port Spain, we crossed the stream of one of the branches of the Orinoco, the temperature of which was 85°·5, and the specific gravity not more than 1·0064; the general surface of the Gulf being 1·0204.

* The particular observations were as follows, and are accompanied by the temperatures of the air, and of the point of deposition, observed at the same hours.

Between Trinidad and Jamaica.					Port Royal, Jamaica.					Between Jamaica and Havannah.				
Oct.	Time.	Water.	Air.	Point of Depos.	Oct.	Time.	Water.	Air.	Point of Depos.	Nov.	Time.	Water.	Air.	Point of Depos.
11	8 A.M.	83°	83·2°	77·5°	20	10 A.M.	In the offing 83°; in the middle of the harbour 81°·5, at sunrise; 83°·5, at 2 P.M.	83·7°	76°	10	9 A.M.	83·1°	82°	°
11	2½ P.M.	83	83	78·5	22	noon		83·5	76·5	10	3 P.M.	83·8	83·2	
12	8 A.M.	83	82	76·5	23	7 A.M.		78·8	76	11	8 A.M.	83	81·8	74
13	8 ...	82·8	83	77·5	23	2 P.M.		82·5	76	13	8½ ...	82·5	80·8	72
14	8 ...	82·9	82	78	24	noon		83	77	14	8½ ...	82·2	79·7	72
15	8 ...	83	82	rain	25	9½ A.M.		81·7	75	14	3 P.M.	82	78	rain
16	8 ...	83	83·4	77·5	29	noon		85·5	78	15	8½ A.M.	80	78·8	72·5
17	8 ...	83	82		30	10 A.M.		84·5	78	17	8½ ...	82	80·3	74·5
17	noon	...	82	77	31	10 ...		83	76	17	2½ P.M.	82·1	80·2	71·5
					Nov.					18	8½ A.M.	80·5	78·9	73
					1	10 ...		82·5	75					
					2	6 ...		78	75					
					3	6 ...		78·5	76					

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on approaching Havannah, on the morning of the 18th, we were apprised, by the colder temperature of 80°·5, that during the preceding night we had entered the current, which descends from the northern shores of the Gulf of Mexico along the coast of Florida; and forms the head of the gulf-stream. In the subsequent passage from Havannah to the Straits of Bahama, on the 27th, 28th, and 29th of November, we crossed the narrow sea formed by the northern shore of Cuba and the Florida reefs, in which the waters of the stream are comprised, previously to their discharge into the Atlantic: the surface-water in this passage varied from 80°·5 to 80°·7, which may therefore be considered as the initial temperature of the gulf-stream towards the end of November. The strait between the Bahamas and the eastern side of Florida, which forms the outlet of the stream, is rather less than 200 miles in length, and from 33 miles at the narrowest part of the water-way, to 50 miles at the widest, in breadth. The Pheasant was at the southern extremity of the strait at noon on the 29th, and at the northern extremity at noon on the 30th, with

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The light rain which fell on the afternoon of the 14th of November, in the passage between Jamaica and Havannah, was a precipitation from an height above the earth's surface, as the air near the surface was very far from being replete with moisture at the time. It was produced by the commencement of a wind from the N.E. (the same, I believe, which is called at Havannah, *el Norte*), which almost instantly lowered the temperature of the air two degrees at the surface, and of course correspondingly in its ascending progression, whilst the dew-point and its progression remained unaltered. The height, therefore, at which the temperatures of the air and vapour would coincide by reason of the difference in their respective ratios of cooling, would at once descend a space equivalent to that required to diminish the temperature of the air two degrees in its ascending progression, and a precipitation would take place throughout that space too copious to be altogether re-dissolved by falling into a warmer atmosphere; and thus some portion of it would reach the surface, forming the light rain which we experienced. It was not, however, of long continuance, the superfluous moisture being disposed of, and the atmosphere speedily adapting itself to the new order of circumstances, by the processes which have been so well pointed out by Mr. Daniell, in his essay on the habitudes of an atmosphere of permanently elastic fluid mixed with aqueous vapour.

I am not able to assign with confidence the cause of the surface-water being only 80° on the morning of the 15th; but I suspect that it evidenced the presence of a thread of the current which descends from the northern shores of the Gulf of Mexico along the coast of Florida; and of which a small portion from the western border is sometimes turned to the westward by the northern coast of Cuba on which it impinges, and takes a course towards Cape St. Antonio. The charge of a convoy in a sea so much infested with pirates, was incompatible with the measures which would have been necessary to have ascertained, more particularly, the cause of the decrease in temperature of the surface-water.

good observations of the latitude on both days, and with especial care given to the intermediate reckoning. The rate of three miles an hour (or more exactly seventy miles in twenty-four hours) may, therefore, be regarded with confidence as the initial velocity of the gulf-stream at that period.

The maximum of its temperature in the strait was 80°·8, and the minimum observed 80°·5; but the Pheasant did not approach the shore on either side, where the surface is known to be colder by reason of the vicinity of land.

The diminution in the rapidity of the stream on the 1st, 2d, and 3rd of December, is the consequence of its expansion after the outfall into the Atlantic; it is probable, however, that on neither of the three days was the Pheasant in the full strength of the current, being nearer the inner border, where the velocity is checked, and the waters accumulated, by the direction of the coast of America between Charleston and Cape Hatteras; the consequence of the accumulation is seen in the increased rate on the 2d and 3rd, in comparison with that on the 1st of December; and in the very remarkable circumstance, that after passing Cape Hatteras, the velocity experienced between the 3rd and the 5th of December was actually greater than the initial velocity at the outlet, being 3·2 miles an hour on the average of the forty-eight hours, or seventy-seven miles in each twenty-four hours; and was, doubtless, considerably greater than the average during a part of the time. The accumulation of the water of the stream in the neighbourhood of Cape Hatteras, to such an extent as to occasion it to flow off with even greater rapidity than on its discharge into the ocean from the Gulf of Florida, is a fact which I believe had not been previously observed, but which may be explained by a brief notice of the different states, at different seasons, of the current, and of the ocean through which it pursues its course. In the summer months, the stream issues from the outlet with a velocity nearly one-third greater than at the period of the Pheasant's voyage; its original northerly direction, received from the Bahama channel, is turned considerably to the eastward of north (about N. 50° E.) by the coasts of Georgia and South Carolina, in which new direction it passes Cape Hatteras, and pursues an unobstructed course, until it impinges upon the St. George's bank to the eastward of Nantucket, by which it is turned still more to the eastward: but as it strikes the bank very obliquely, it is deflected without material accumulation of its water, or increase of velocity. The St. George's bank is the last obstruction that the stream encounters, as it never afterwards approaches land. There is, therefore, no accumulation in the summer months in the neighbourhood of Cape Hatteras; but on the contrary, the western border of the stream expands into the great bay between Cape Hatteras and Nantucket, and occasions a diminution rather than an increase in the velocity at the surface; accordingly it is found that the force originally communicated at the outlet is progressively diminished from above eighty miles in twenty-four hours in the first 180 miles after its discharge into the Atlantic, usually to less than seventy miles when abreast of Cape Hatteras.

On the approach of winter, the disparity in the general level of the Gulf of Mexico and the Atlantic, is diminished by the reduction in the level of the Gulf, and the impulse communicated to the stream at its fall into the Atlantic is proportionably lessened. At that season, also, an alteration takes place in the level of the part of the ocean towards which the course of the stream is directed. The heavy autumnal gales from the north and north-east impel before them the superficial waters of the north-western Atlantic into the space comprised between the coast of America and the gulf-stream: this space, which is of considerable width between Cape Race in Newfoundland and the northern border of the stream, narrows towards the westward, and has no outlet; the drift water consequently accumulates, and presses wholly against the northern and western borders of the current, and by raising the usual level of the ocean, prevents the surface-water of the stream from reaching the Nantucket and St. George's banks, and opposes the expansion of the western border into the recession of the coast of the continent between Cape Hatteras and Nantucket; the accumulation of the gulf-water is thus occasioned, which streams off to the north-east with the augmented velocity experienced by the Pheasant between the 3d and 5th of December. It is probable that the occasional effects thus noticed are very superficial, and that the great body of the water which issues from the Gulf of Florida, and is of considerable depth, is governed, both in direction and velocity, solely by the original impulse, and the banks on which it impinges; but navigation is more immediately concerned with the surface-current only.

On the 5th of December, between 10 A.M. and noon, the Pheasant quitted the gulf-stream, passing out on its northern side. At 8¼ A.M. she was in longitude by observation 72° 25′ W., and in latitude, deduced from the subsequent noon, 36° 14′; the temperature of the surface-water was 74°, and of the air 60°·5. At 10 A.M., the temperatures being still the same, the depression of the horizon, observed with a dip-sector from the Pheasant's gangway, where the height of the eye was 15 feet 3 inches above the sea, was 4′ 56″·6, being an excess of 1′ 05″·6 above the usual computed and tabular depression.

On repeating the observations at noon, it was found that a change of great magnitude had taken place intermediately; the horizon, viewed from the same height, making an angle on the second occasion, of only 3′ 36″·6 with the horizontal line passing through the eye. As the ship was in both instances very steady, and the horizons perfectly clear, the observations were decided and certain; and the utmost error of which either might be suspected could not be more than 5″. So great an alteration in the refractive quality of the atmosphere led to the immediate suspicion, that the temperature of the surface-water of the sea must also have greatly altered, and that we must have passed from the warm water of the stream into the colder surface of the general ocean. This suspicion was confirmed on trial, the temperature having fallen from 74° at 10 A.M. to 62°·4 at noon, being a difference of 11°·6. As a measure of precaution on such a sudden and great decrease, Captain Clavering immediately sounded, but had no bottom with 120 fathoms: the temperature at 110 fathoms, indicated by a register-thermometer attached to the line above the lead, was 51°·5. The distance from the nearest banks noticed in the charts was sixty-five miles.

The northern boundary of the stream, where we had thus quitted it, was between the latitudes of 36° 26′ and 36° 38′, and in the meridian of 72° 30′ W. The surface-water on which we entered was in motion to the westward, at the average rate of sixteen miles, experienced in the following twenty-four hours, and generally to the west and south-west between the northern side of the stream, and the banks on the coast of Maryland. This motion may be more properly characterized as a drift-current, occasioned by the prevalence and strength of recent northerly gales, than as a counter-current. In approaching the banks, the surface-water at 8 A.M. and at noon on the 7th of December was 59°·5; at 3 P.M. it had fallen to 54°·2, on which we immediately sounded and found bottom in thirty-three fathoms: on the following morning in thirty fathoms, the surface was 53°·5, and at 8 A.M. on the 9th in twelve fathoms, but still with no land in sight (being twenty miles off the coast) 49°·5. In the afternoon of the same day, when about two miles distant from Sandy Hook, the water had finally lowered to 45°. Thus in a space of the ocean scarcely exceeding 200 miles in direct distance, we found the heat of the surface progressively diminished from 74° to 45°.

On a general review of the influence of the currents which have been thus particularized, on the Pheasant's progress in her voyage commencing at Sierra Leone, and terminating at New York, it may be seen, that she was indebted to their aid on the balance of the whole account, and in the direction of her course from port to port, not less than 1600 geographical miles, the whole distance being under 9000 miles; affording a very striking exemplification of the importance of a correct knowledge of the currents of the ocean to persons engaged in its navigation; and consequently of the value of the information, in the acquisition and arrangement of which Major Rennell has passed the later years of his most useful life. The publication of the charts of the currents in the most frequented parts of the ocean, which he has prepared with his accustomed and well-known indefatigable assiduity, and strict adherence to the evidence of facts,—as soon as he shall deem them sufficiently complete for the public guidance,—will be a most important service rendered to practical navigation.

LXIII. *Notice relating to the Theory of the Equilibrium of Fluids. By* J. Ivory, *Esq. M.A. F.R.S.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

As I find that what I have written on the equilibrium of fluids is misrepresented on some occasions, I must beg leave to occupy a small space in your Journal, in stating correctly the points I have endeavoured to establish.

In the Philosophical Transactions for 1824, and in the pages of your Journal, I have treated specially of a homogeneous fluid. The equilibrium in this case, according to what is laid down in all the books, is contained in a single law; namely, that the gravitation at the outer surface be directed inward at right angles to that surface, at the same time that the differential equation is integrable without supposing any relation between the coordinates. That this is a correct account of the received theory I need only cite the *Mécanique Céleste*, Liv. 1^{me}. Nos. 17 and 34. Liv. 3^{me}. No. 22. No condition is required with respect to the interior of the fluid, and the reason is expressly given. *Comme on peut dans l'interieur d'une masse homogene, prendre telles couches que l'on veut, pour couches de densité constante; la seconde des deux conditions precedentes de l'equilibre, est toujours satisfaite, et il suffit pour l'equilibre, que la première soit remplie; c'est-a-dire que la resultante*

sultante de toutes les forces qui animent chaque molécule de la surface exterieure, soit perpendiculaire à cette surface.—Méc. Cél. Liv. 3^{me}. No. 22. Having now described accurately the received theory of the equilibrium of a homogeneous fluid, I have next to add, that I found it defective and insufficient. The defect arises from neglecting to consider the level strata in the interior of the mass. These strata are derived from the equation of the outer surface in the same manner in a homogeneous fluid as in one of variable density. Their mathematical definition is the same in both cases, although the physical distinction arising from difference of density which obtains in one case is lost in the other. They are not more arbitrary when the fluid is homogeneous than when its density varies from one stratum to another. Viewing the matter in this light, I supplied the defect of the received theory in two different ways: First, by the principle that every level stratum attracts a particle in the inside with equal force in opposite directions: Secondly, by this other principle or theorem, *If a homogeneous fluid body revolving about an axis, be* in equilibrio *by the attraction of its particles in the inverse proportion of the square of the distance; any other mass of the same fluid having a similar figure and revolving with the same rotatory velocity about an axis similarly placed, will likewise be* in equilibrio, *supposing that its particles attract one another by the same law.*

The second principle is susceptible of a very clear demonstration, to which no objection can be made. If we take any two particles placed alike in the two bodies, the attractive and centrifugal forces will have the same invariable proportion; namely, that of the linear dimensions of the bodies. Thus the forces that act [illegible] the particles of one body, are all augmented, or all diminished, in the same constant ratio in the other body; wherefore if there be an equilibrium in one case, there must likewise be an equilibrium in the other case. Hence it is easy to deduce, that when a mass of a homogeneous fluid is *in equilibrio*, all the level surfaces must be similar to one another; and this is sufficient to determine *à priori* the figure which the body will assume.

With regard to the first principle, I reasoned in the manner following. The pressure which any exterior level stratum exerts upon the fluid below it, must be perpendicular to the separating surface. But this pressure is the effect of two distinct causes. One is the combined effect of the attraction of the interior matter and the centrifugal force, which two forces constitute the gravitation at the separating surface; the other is the attraction of the stratum upon the interior matter. Now it is a mathematical property of the level surfaces, that the gravitation at any such surface causes an exterior level stratum in contact with it to press perpendicularly upon the interior fluid; whence it follows that the attraction of the exterior matter upon the interior fluid must likewise cause a pressure perpendicular to the separating surface. The interior fluid must therefore be *in equilibrio* by the attraction of the exterior stratum; which can happen only because every particle of the interior fluid is urged equally in opposite directions by the attraction of the stratum. It will be allowed that the equilibrium in question is a necessary consequence of the cause assigned; and when we consider that any particle within the stratum, is on every side urged outward by the attraction, it seems impossible not to admit the soundness of the principle. It must be recollected that, in all this, we are here speaking only of a homogeneous fluid; and we may even confine our attention to the equilibrium of such a fluid acted upon by the attraction of its particles in the inverse proportion of the square of the distance and by a centrifugal force. In this view there is no doubt of the truth of the first principle, for it is easily deduced from the second. Either of them is sufficient to determine the figure of equilibrium *à priori*, and they prove that it can only be an oblate elliptical spheroid.

It is a great mistake to suppose that it is intended to advance any thing in opposition to the general equation of equilibrium, in which all the forces in action are taken into account. But that general equation will not apply to particular problems without some modifications which it is necessary to investigate.

It is of great consequence to determine the figure of a homogeneous fluid *in equilibrio*, because that of a heterogeneous mass will depend upon it, and must be deduced from it. In a body *in equilibrio*, which consists of strata of variable density, it is obvious that very near the centre we may consider the density as uniform or constant. This small central body of constant density, being *in equilibrio* by the attractions of its particles and a centrifugal force, will therefore have the figure of an oblate elliptical spheroid. It is obvious that we may suppose it to become hard and solid, and then it will form a nucleus upon which all the superior strata will press and rest. Supposing now that the nucleus is given, we must determine the figure necessary to the equilibrium of every one of the superior strata; which requires that a proper form of expression be given to the radius of every stratum. Let $\varphi^{(2)}$, $\varphi^{(4)}$, $\varphi^{(6)}$, &c. denote functions of 2, 4, 6, &c. dimensions of three rectangular coordinates of a sphere, that is, of the quantities,

$$\mu, \ \sqrt{1-\mu^2}\cos\varpi, \ \sqrt{1-\mu^2}\sin\varpi;$$

then, r being the radius of a stratum, we shall have,

$$r = a\left\{1 + e\varphi^{(2)} + e^2\varphi^{(4)} + e^3\varphi^{(6)} + \&c.\right\},$$

e being a quantity according to which the steps of approximation are to be arranged. This general expression supposes that the central nucleus is an ellipsoid; but if it be a spheroid of revolution, then we must make $\varphi^{(2)}$ equal to μ^2, or $1-\mu^2$; and $\varphi^{(4)}$, $\varphi^{(6)}$ &c. must be trinomial, quadrinomial, &c. functions of μ^2. The quantity c, the density of the stratum, and the coefficients of all the angular quantities are functions of a; and they must be determined so that a stratum shall satisfy the conditions of equilibrium, and, when it comes to the centre, shall rest upon the solid nucleus. In this manner we shall have the most general solution of the equilibrium of a heterogeneous mass of fluid, pushed to any proposed degree of approximation. I am, &c.

June 14, 1826. J. IVORY.

LXIV. *Supplement to Mr.* HERAPATH'S *Paper in the Philosophical Magazine for August* 1825, *on Functional Equations. By* JOHN HERAPATH, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

FROM the period of sending my last communication to the Phil. Mag., my mind has been so much estranged from the consideration of functions, that I might almost be said to have entirely abandoned them. This is the reason why so long a time has elapsed without my having noticed the omission of a restriction, which, on reperusing the paper alluded to a few days since, I was surprised to find I have no where distinctly mentioned. It should have been stated, that *the arguments of the paper relate to those periodical functional equations only, whose solutions contain arbitrary functions.* We may indeed understand this from "*arbitrary functions,*" "*complete solutions,*" &c. in the heading of the paper; but as there are an infinite number of periodical functional equations, having their solutions complete, without containing arbitrary functions, and therefore wanting the properties which render our reasoning applicable, I hasten to supply the omission.

As an example will best illustrate the propriety and necessity of the above restriction, let us take our old equation

$$\psi x = fx \, . \, \psi \alpha x + f_{\prime} x, \qquad (1)$$

supposing the only condition for the present to be $\alpha^2 x = x$, and leaving fx and $f_{\prime} x$ entirely unlimited. By changing in (1) x into αx and eliminating between the resulting equation and (1) $\psi \alpha x$, there is obtained

$$\psi x = \frac{f_{\prime} x + fx \, . \, f_{\prime} \alpha x}{1 - fx \, . \, f \alpha x}; \qquad (2)$$

which is the complete solution of (1) without an arbitrary function. To prove this, let us add some function φx to the solution (2), which for brevity we denote by Px; and it is evident that if Px be not the complete solution, φ may have such a form that

$$\mathrm{P}x + \varphi x$$

shall contain it, or at least some one of the other solutions. Substitute Px + φx and Pαx + $\varphi \alpha x$ for ψx and $\psi \alpha x$ in (1), and we get

$$\mathrm{P}x + \varphi x = fx \, . \, \mathrm{P}\alpha x + fx \, . \, \varphi \alpha x + f_{\prime} x;$$

or since by (2) $\quad \mathrm{P}x = fx \, . \, \mathrm{P}\alpha x + f_{\prime} x$

$$\varphi x = fx \, . \, \varphi \alpha x.$$

Changing in this equation x into αx it becomes $\varphi \alpha x = f \alpha x \, . \, \varphi x$, and of course gives

$$\varphi x = fx \, . \, f \alpha x \, . \, \varphi x;$$

an equation in which, if fx be as we suppose it unlimited, φx must be null, and consequently Px or (2) is the complete solution. But if φx be assumed to have a real value, then must $fx \, . \, f \alpha x = 1$, and as a consequence by (2) $f_{\prime} x = - fx \, . \, f_{\prime} \alpha x$; which are what I have termed in my above-cited paper the *conditions of possibility.*

The trouble of working out and printing the solution of a periodic of a higher order than the second, is the only reason why I do not here avail myself of it in corroboration of the position advanced.

From what has been shown, there is manifestly a marked distinction between periodical equations considered indefinitely, and those of which I have treated in my paper. In the former, single solutions flow from the given functions being indefinite; in the latter, innumerable solutions are the consequences of certain restrictions in the functions proposed. For the sake of distinction I shall hereafter call the former simply *periodic functions*, and the latter *porismatic periodical functions.*

It would be hazardous in subjects so very general to pronounce positively, that the above distinction is universally true. Possibly there may exist certain equations, perhaps partially indefinite, which may contain in their solutions functions partially arbitrary, or arbitrary between certain limits; but I confess I do not know of any such.

Though Mr. Babbage and others have solved several equations, both simple and porismatic, I do not know that this essential line of distinction has occurred to any of them. As a proof, however, of the importance of well understanding the difference between these two species of equations, I may observe, that notwithstanding their solutions are obtained by means so very similar, yet the solution of neither species, as it appears to me, contains nor is contained in that of the other. For example (2), which is the solution of (1) when $fx, f_{,}x$ are indefinite, neither contains nor is contained in the solution of the same (1), when this equation is limited to the conditions of possibility, namely $fx \,.\, f\alpha x = 1$ and $f_{,}x = -fx \,.\, f_{,}\alpha x$.

By the above considerations the solution of

$$f\{x,\ \psi x,\ \psi\alpha x,\ \psi\alpha^2 x,\ \ldots\ldots\} = 0, \qquad (3)$$

taken generally, has but one form, not accounting of course for any thing those changes which the double signs of radical quantities may occasion. This single solution is in general to be deduced from the elimination of $\psi\alpha x,\ \psi\alpha^2 x, \ldots$ between the equations which result from the successive substitution of $\alpha x,\ \alpha^2 x, \ldots$ for x in the primitive equation.

The same observations extend to

$$f\{x,\ \psi x,\ \psi^2 x,\ \ldots\ldots\} = 0. \qquad (4)$$

For since $\psi^r x$ can always be equated with $\phi\alpha^r\phi^{-1}x$, in which α may have any given form at pleasure, provided only that α be confined to the same order of periodicity as ψ is, we may transform, by putting $\phi\alpha\phi^{-1}x,\ \phi\alpha^2\phi^{-1}x, \ldots$ for $\psi x,\ \psi^2 x, \ldots$ and then changing x into ϕx, our (4) into

$$f\{\phi x,\ \phi\alpha x,\ \phi\alpha^2 x,\ \ldots\ldots\} = 0,$$

in which ϕ is the form to be determined. This equation is similar to (3), and consequently in a general point of view limits ϕ, and hence likewise ψ, to one form. It is obviously essential in this solution that we have the order of periodicity of ψ given, otherwise the problem cannot generally be solved; and that α must be of precisely the same order. The gentleman who first treated of the solution of (4) seems not to have included the *order* of periodicity of ψ amongst the necessary data; and seems to *have imagined that if αx be not taken $= x$ its order is not of great consequence*, as it would merely limit the generality of the solution. It was, however, not then known that an indefinite form to f gives but one solution, which therefore cannot be further limited.

Cranford. J. HERAPATH.

P. S. Since writing the above, I have turned my attention to the solutions of non periodical functional equations and other subjects connected with them; in which, with the help of an improved notation, I have met with a degree of success beyond what my most sanguine expectations could have anticipated. Indeed, if the views which have opened to me, should turn out as prolific as they seem to promise to be, they will entirely new face most of the instruments of analysis, and will leave but little to desire except the perfection of common algebra.

LXV. *On the Ignition of Gunpowder by the Electric Discharge; and on the Transmission of Electricity through Water, &c. By Mr.* W. STURGEON.

To the Editor of the Philosophical Magazine and Journal.

Sir,

IT is generally admitted, that the present state of knowledge relative to the phænomena of electricity is enveloped in much obscurity; and perhaps no instance of electrical action manifests our ignorance of this branch of science more, than that of igniting gunpowder. Yet, so little notice is taken of this isolated fact, that no satisfactory attempt at explanation, that I am aware of, has ever appeared in the pages of any writer on this subject.

That gunpowder has frequently, by various individuals, been ignited by the electric fluid, is a truth that cannot be denied. But *why* those experimenters happened to succeed, and *why* others so frequently and still more constantly *fail*, are circumstances the cause of which has hitherto been left unexplained,—perhaps not understood. I am well convinced that no individual experimenter has been more embarrassed than myself, by fruitless attempts to ignite gunpowder by the electric fluid: and although I have varied the experiment according to all the directions I could either read or hear of, yet I candidly confess that I never succeeded by any of them.

Fruitless as these experiments were with respect to the object in view, it was observed, by passing the discharge of a jar through water (which is the method given by some authors), that the force of the shock is considerably abated; and that the report is very trifling, when compared to that which is heard by a similar discharge through metal. Convinced by this circumstance that the nature of the discharge is *modified* in a peculiar manner by passing the fluid through water; yet, as I had never succeeded in igniting gunpowder by a discharge through the aqueous medium, it appeared evident that something more than this fact was necessary to be understood. I therefore became desirous to ascertain, if possible, the real cause why other experimenters succeeded, and why every attempt that I had made proved unsuccessful.

It is well known, that if a red-hot iron be applied to gunpowder, the latter does not immediately ignite, but that some interval of time (however small) does certainly elapse before one single grain is on fire: and that it is possible for a red hot iron to be passed over the hand with such velocity, as to produce scarcely any sensation of heat. Hence my first object, now, was to devise some means of retarding the velocity of the electric fluid: for I considered that if this could be accomplished, more time would be afforded for the fluid and gunpowder to be in contact, and the latter, in consequence, more likely to be ignited.

I likewise supposed that if the electric fluid be retarded by being transmitted through water, it was likely that a jar would not be quite discharged by a very sudden contact; as it was probable that if the discharging rod was quickly withdrawn from the knob of the jar, that the whole of the fluid would not have time to make its escape. But several trials in this way, through a large tub of water, seemed to discharge the jar as effectually as if the whole circuit had been of metal.

Although these experiments were by no means satisfactory, yet I always observed that the report was much feebler, and that gunpowder placed in the circuit was not blown or scattered to so great a distance, when the discharge was made through water, as when a similar discharge was transmitted through metal. Hence it was obvious, that the force had by some means abated; but whether by retardation, or by some change in the physical character of the electric fluid, I was at that time unable to determine.

Some time afterwards, an idea suggested, that if the electric fluid be retarded at all by passing through water, perhaps the water possesses this property in consequence of its inferior conducting capacity with respect to metals and other good conductors; and if so, the velocity of the electric fluid might be reduced to almost any degree, by reducing the diameter of the column of water through which it had to pass. For it is evident that the conducting power of any body will be proportional

portional to its natural capacity, and to the quantity employed at any one point in the circuit. For a discharge that will destroy a thin wire, would be conducted with safety by a wire of the same kind of metal, of greater dimensions. It now occurred, that those persons who had ignited gunpowder by the electric fluid, perhaps succeeded by using very narrow tubes filled with water. (For I had frequently transmitted a discharge through a wide tube without success; and as no author gives any dimensions of the water employed, it did not till now occur, that the time of transmission would vary with the calibre of the tube.) I had not at this time any narrow tubes in my possession. Considering, however, that if any non-conducting substance—such as silk, or paper,—were moistened with water, that those substances could have no more conducting power than what was imparted to them by the moisture; my first experiment was with a single thread of sewing-silk about four inches long, well moistened by drawing it between my lips. This thread was made a part of the circuit between the inside and outside of a charged jar. At another part of the circuit an interruption was made between the extremities of two wires; and at this interruption was placed some gunpowder. On discharging the jar the gunpowder ignited. I repeated the experiment several times with the same success. I afterwards varied the experiment, by using the same thread and a smaller jar, and succeeded in igniting gunpowder with about thirty inches of charged surface. I must here observe, that when the thread was very wet, I never succeeded with this small jar, owing, as I suppose, to the quantity of water contained in the thread being too great to retard the small quantity of electric fluid contained in the jar. For by squeezing out some of the moisture, the thread became a worse conductor, and then I always succeeded.

I next tried how far it was possible to succeed with the first jar, and augmenting the quantity of water. For this purpose a piece of twine was used, well soaked in water. This twine, however, conducted the electric fluid with too much facility to ignite the gunpowder; but when some of the moisture was squeezed out, it answered very well. Thus, by proportioning the one with the other, I always succeeded. The same results were obtained by using moistened paper.

Having satisfied myself on this point, I next endeavoured to ascertain if the electric fluid undergoes any change in its physical character by passing through water; or if the ignition of gunpowder depends entirely on the time occupied by the fluid to pass through it. For this purpose I employed two jars, which for distinction we will call A and B. I charged A positively, and B negatively; and connected their outsides by water. On exploding A into B, through water, both jars became neutralized. A was again charged positively, and afterwards partly discharged through water into B. On discharging both jars separately, there appeared no difference in the explosions. A [illegible] once more charged positively, and again partly discharged through water into B. On discharging B through a moistened thread, gunpowder was ignited in the circuit. These experiments were reversed, by charging the jar A with negative instead of positive electricity, and the results were similar. Hence I concluded, that the ignition of gunpowder by the electric fluid depends on the time occupied by the latter in passing through it, and not on any change in the physical character of the fluid. Hence also, by the foregoing experiments (when the intensity of the charge is constant), the time occupied by any given quantity of the electric fluid, in passing any one point in the circuit, will be in some reciprocal proportion* to the thickness of the column of water employed in that circuit.

It was observed in these experiments, that the moistened thread soon became nearly dry. Hence the electric fluid had either decomposed the water or caused it to evaporate. The former effect I suppose to have taken place, which, if true, perhaps the decomposition of water by this agent may be facilitated by reducing the diameter of the column employed. I have not yet had time to ascertain this particular satisfactorily, therefore it remains a mere supposition.

I have frequently discharged a jar through my own body without any other inconvenience than a burning sensation at the extremities of the fingers; and have ignited gunpowder in the same circuit.

So modified is the electrical discharge by being transmitted through aqueous conductors, that the effect of an intense charge of the most powerful battery may be reduced to almost any degree. I have discharged eight feet† of charged surface through my own body without feeling the least shock. But the burning sensation was very severe.

In medicine, this modification of electricity can hardly fail to be useful; for it may be administered to any particular part of the body without affecting any other part. It may be applied to the skin of the most delicate patient; and without the least danger of giving a shock, a most powerful stream of electricity may be poured on the part affected.

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* This proportion may vary either as the diameter, or as the square of the diameter of the column: according as the electric fluid occupies the surface, or the whole body of the water; and if the velocity of each individual particle of the electric fluid be affected, the transmission of the whole mass will also vary on that account.

† Eight feet of lining, and eight of coating.

It has been also ascertained, that the force of an electric discharge through metal, is always proportioned to the thickness of the wire through which it is transmitted; or, that the same quantity and intensity of electric [illegible] acts with a greater force when transmitted through a thick [illegible] through a thin wire.

This law of course has a limit; [illegible] the conducting wire be sufficiently stout or capacious to transmit the fluid without interruption, a wire of larger dimensions can give no more facility to the transmission. I am of opinion, however, that thick wire facilitates the transmission of the electric fluid to a greater degree than is generally suspected.

Now it is evident, that as thin wire has the property of diminishing the intensity of an electrical discharge, the fluid during its transmission through such a wire must necessarily be drawn out (as it were) into a longer stream, than if it were transmitted through a thick one. In the same manner that a certain quantity of water would be drawn out into a longer stream, by passing through a narrow than through a wide tube; or as a piece of metal would be drawn into a longer wire by passing through a small than through a large hole in the plate.

Hence it became a curious question, What length of wire of a given dimension, does a certain quantity of the electric fluid (say a jar charged to a certain intensity) occupy, at any moment during its transmission? I supposed that this might probably be ascertained by placing some gunpowder at an interruption near to the positive side of the jar, and the moistened thread near to the negative side, having a long copper wire between them. For if the wire was of sufficient dimensions to contain all the fluid at once, the latter would not meet with any resistance till it arrived at the moistened thread; and the time of its transmission through that part of the circuit where the gunpowder was placed, would not be lengthened; upon which supposition, the gunpowder ought not to be ignited.

With such arrangements, I have separated the gunpowder and moistened thread by a copper wire, No. 16, of different lengths, from one to twenty yards; yet with all this length of good conducting substance beyond the gunpowder, the latter never failed to be ignited.

I consider this a very curious circumstance, and the inquiry important. Not so much because the gunpowder always ignited in these experiments (for it is possible that with longer and stouter wire, and a smaller charge, the ignition might not take place); but because, if it could be proved by experiment that the electric fluid would, by intervening capacious good conductors, ignite gunpowder at the negative, and not at the positive side of the moistened thread, such experiment would prove to demonstration the truth of the Franklinian hypothesis.

I am well convinced that if the electric fluid ever passed the gunpowder without interruption, the latter could not be ignited by any recoil of the fluid into the jar from the interrupting moistened thread; because, if ever it passed through the gunpowder with violence, it would scatter, or blow the latter substance away, so that none would remain in the circuit to be ignited at the time of the fluid's return.

Several experiments on the ignition of gunpowder by the electric fluid were exhibited, and demonstrated on the foregoing principles, in a lecture which I had the honour to deliver before the members of the Western Literary and Scientific Institution, on Monday evening last (May 8th) in the Concert Room, King's Theatre, Haymarket. In one experiment the gunpowder was ignited at one extremity of the moistened thread; and in another experiment four guns were fired in the same circuit. I have frequently fired six guns by one discharge of a jar; and so instantaneous is the ignition at the several guns, that their united reports appear like the report of one gun only.

I am at present engaged in other experiments on this branch of electricity, and shall not fail to communicate to the public the results of such as appear worthy of notice.

I am, sir, your's faithfully,

Artillery Place, Woolwich, W. STURGEON.
May 15, 1826.

LXVI. *Proceedings of Learned Societies.*

LINNÆAN SOCIETY.

June 6.—READ a paper by the Rev. Lansdown Guilding, F.L.S., On a new Genus of Insects named *Oiketicus.* These insects exhibit a singular peculiarity in the *sponsalia.* The female never leaves the pupa-case. They seem to be allied to the genus *Psyche,* and their œconomy serves to illustrate the mode of propagation in this last genus, which has been asserted to be without any sexual intercourse.

Read also, a paper by the Secretary, J. E. Bicheno, Esq., On Methods and Systems in Natural History; wherein the author endeavoured to show the different uses to which they should respectively be applied. The chief object of the Artificial system, he insisted, was to analyse; that of the Natural system, to synthesise. The business of the one is to enable us to ascertain particulars; and of the other, to combine those particulars so as to assist the mind to reason generally. Systematists in general, he contended, have confounded these two distinct objects, and have attempted to employ their natural systems equally with a view to determine species as to combine them, while their chief object should have been to find resemblances and common characters. The state of science seems to require that the work of combination should be more studied; and that instead of breaking down the productions of nature into the smallest particulars, we should act more philosophically if we endeavoured to discover the common characters of her groups, and to unite species; and thus furnish the ordinary reader with materials of knowledge, relieve his memory, and abridge his labour. This seems to be the more necessary in the present day, when the number of birds amounts to 5000, of insects to 100,000, and of flowering plants to 50,000.

June 20.—The following papers were read: Concise notice of a species of *Ursus* from Nipal, a skin of which has been presented to the Linnæan Society by H. T. Colebrooke, Esq. &c. &c.; by Thomas Horsfield, M.D. F.L.S. Dr. Horsfield characterizes this animal as follows: "Ursus sordidè fulvus nitore isabellino, pilis collo dorsoque elongatis, molliusculis, crispatis; ad latera rigidis adpressis; unguibus brevibus, rectis, obtusis." It seems to agree with the European rather than with the Asiatic bears.

Description of a new British Freshwater Helix; by the Rev. Revett Sheppard, M.A. F.L.S.

Of the term *Oistros* or *Oestron* of the ancients, and of the real insect intended by them in this expression; by Bracy Clark, F.L.S. Foreign Member of the Academy of Sciences of Paris, and of the Naturforchender Freunde Society of Berlin, &c.

It is affirmed in this paper that the *Œstrus* Linn., and not the *Tabanus* as Mr. W. MacLeay contends, is the real *Œstrus* of the Greeks, and *Asilus* of the Romans.

ASTRONOMICAL SOCIETY OF LONDON.

May 12.—A paper, by the Astronomer Royal, was read, containing an explanation of the method of observing with the two mural circles, as practised at present at the Royal Observatory. The principal object of the method explained in this paper is to diminish as much as possible the inaccuracies occasioned, even in the most perfect instrument, by rapid and partial changes of temperature. In the Greenwich system of observations, assistance from the spirit-level or plumb-line, or indeed from any previous verification, is rejected altogether. Two circles are employed simultaneously, each of which is furnished with six microscopes, which it is desirable should be placed at *nearly* equal distances on the limb; and previously to observation each circle is placed *nearly* in the plane of the meridian, and *nearly* perpendicular to the horizon. Each circle is provided with an artificial horizon of mercury, so as to command the greatest possible portion of the reflected meridian.

The first part of the process consists in observing a number of stars simultaneously with each instrument, either by direct, or by reflected vision; the object of this is to determine the exact quantity that one instrument marks more or less than the other, when both are directed to the same object. This is determined not by a single observation, but by a great variety; thus obtaining the quantity denominated *the mean difference* for every 24 hours.

In the second part of the process, a series of stars is observed *reciprocally*, that is, the direct image of a star by one instrument, at the same time that its reflected image is observed by the other. This, combined with the results of the previous process, in which the *mean difference* serves the same purpose as the index-error in Hadley's sextant, enables the observer to ascertain the altitude; with which is likewise obtained the knowledge of the position of the horizontal diameter of each instrument. The observer, however, does not rest contented with a single determination of one diameter; but must in a similar manner, from altitudes, observed on various points of the arc, and by taking sometimes the direct, and sometimes the reflected observation with the same instrument, endeavour by every possible variety to obtain the maximum of precision of which the method is capable.

The position of the horizontal diameter of each instrument being thus deduced from a mean of all the preceding experiments, sufficient data are obtained for computing the places of those stars that have been observed in the first part of the process, and employed in computing the mean difference; because without the knowledge of the position of their horizontal diameters, the instruments, with respect to the stars in question, give nothing but differences of declination; but such position being known, their altitudes can be accurately determined.

The Astronomer Royal terminates his paper, by pointing out the principal advantages of the method described.

There were next read Extracts of three letters addressed by M. Gambart, Director of the Observatory of Marseilles, to James South, Esq. respecting the discovery and elements of the orbit of a comet, supposed to be the same with that, or those, of 1772 and 1805. M. Gambart first presents the summary of his observations of this comet from the 9th to the 21st (inclusively) of March this year. He then exhibits the elements as computed from these observations upon the parabolic hypothesis: viz.

Passage of the perihelion, March 1826, 18,94 days, counting from midnight.

Perihelion distance . . .	0·961		
Long. perihelion . . .	104°	20′	0″
Long. ascend. node . .	247	54	10
Inclination	14	39	15

Motion direct.

These elements were communicated March 23rd:—a week after, the elliptic elements deduced from the same observations were transmitted, and are as follows: viz.

Passage of the perihelion, March 1826, 19,5998 days, counting from midnight.

Semi-axis major	3·567		
Excentricity	0·74187		
Log. mean motion . . .	2·7326487		
Long. perihel.	108°	54′	19″
Long. asc. node . . .	249	55	23
Inclination	13	50	47

Motion direct.

Periodic time	6·567 years.

The same elements, M. Gambart observes, represent almost exactly the observations of the comets of 1772 and 1805; whence the identity of all three is inferred.

The reading of Mr. Herschel's paper on Double-stars, commenced at the last meeting, was continued.

LXVII. *Intelligence and Miscellaneous Articles.*

LIEUT. DRUMMOND'S GEODESICAL INSTRUMENTS.

WE have been favoured with the following abstract of Lieut. Drummond's paper on this subject, read before the Royal Society, on May 4th, and briefly noticed in our last No. p. 373.

In this paper two methods are described by which geodesical operations may be facilitated to a very considerable extent, —the one applicable by day, the other by night. The first, which consists in employing the reflection of the sun from a plane mirror as a point of observation, was first suggested by Professor Gauss; and the result of the first trials made in the survey of Hanover proved very successful. Recourse was had to this method on some occasions that occurred in the Trigonometrical Survey of England, where, from peculiar local circumstances, much difficulty was experienced in discerning the usual signals.

Even as a temporary expedient, and under a rude form, viz. that of placing tin plates at the station to be observed in such a manner that the sun's reflection should be thrown towards the observer at a particular time, the most essential service was derived from its use; and the consequence was, the invention of a more perfect instrument, of which a description is given, accompanied with a drawing.

The second method consists in the exhibition of a very brilliant light at night. At the commencement of the Survey of England, General Roy had recourse, on several occasions, and especially in carrying his triangles across the Channel, to the use of Bengal and white lights; for these, parabolic reflectors illuminated by Argand lamps were afterwards substituted as more convenient; but from want of power they appear in turn to have gradually fallen into disuse. With a view to remedy this defect, a series of experiments was undertaken by Mr. Drummond, the result of which, was the production of a very intense light, varying between 60 and 90 times that of the brightest part of the flame of an Argand lamp.

This brilliant light is obtained from a small ball of lime about 3-8ths of an inch diameter, placed in the focus of the reflector, and exposed to a very intense heat by means of a simple apparatus, of which a description is given, with drawings. A jet of oxygen gas directed through the flame of alcohol is employed as the source of heat. Zirconia, magnesia, and oxide of zinc were also tried; but the light emanating from them was much inferior to that from lime. Besides being easily procured, the lime admits of being turned in the lathe, so that any number of the small focal balls may be readily obtained, uniform in size, and perfect in figure. The chemical agency of this light is remarkable, causing the combination of chlorine and hydrogen, and blackening chloride of silver. Its application to the very important purpose of illuminating light-houses is suggested, especially in those situations where the lights are the first that are made by vessels arriving from distant voyages.

Both the methods now described, for accelerating geodesic operations, were resorted to with much success during the last season in Ireland; and on one occasion, where every attempt to discern a distant station had failed, the observations were effected by their means, the heliostat being seen during the day, when the outline of the hill ceased to be visible, and the light at night being seen with the naked eye, and appearing much brighter and larger at the distance of 66 miles, than a parabolic reflector, of equal size, illuminated by an Argand lamp, and placed nearly in the same direction, as an object of reference, at the distance of 15 miles.

Results of a Meteorological Journal for May 1826, *kept at the Observatory of the Royal Academy, Gosport, Hants.*

General Observations.

This month has been fine and generally dry, with a long continuance of North and North-east winds, which brought on hoar-frost before sunrise almost every morning till the middle of the month. Till the 18th instant the air was remarkably dry, and for the preceding three weeks scarcely enough rain fell to moisten the ground. This dry period, with unobstructed sunshine, was the means of dust accumulating so much on the main roads, as to render travelling uncomfortable at this pleasant season.

The warm showers of rain since the 18th have changed the appearance of the parched grass-lands to a fine green: the corn blades, however, retained their verdant colour throughout the month, and look promising. The effects of the frosts the latter end of last month and the first part of this, are visible both on the wall-fruit and other trees, and also in the appearance of the potato-tops in many districts, although they are now recovering.

The mean temperature of the external air this month, is one degree and a half under the mean of last May, and nearly half a degree under the mean of that month for the last ten years. But during the last few days summer appears to have suddenly opened upon us by an increased temperature, with mild and refreshing showers.

The temperature of spring-water has increased about one degree this month.

In the evening of the 21st, the Chaffers were on the wing, being the first time they had been seen this spring. The same evening the full moon rose in a striking manner behind purple surmounted by red haze (or descending mist so tinged by the horizontal rays of the sun and spreading a blush on the twilight), which gave to her large disc a blood-red colour.

The atmospheric and meteoric phænomena that have come within our observations this month, are one anthelion at 2 o'clock in the afternoon of the 12th, two parhelia in the morning of the 9th; three solar halos, five meteors, one rainbow, lightning in the evenings of the 1st and 10th, distant thunder in the afternoon of the 24th; and two gales of wind, one from the North, the other from North-east.

Numerical Results for the Month.

		Inches.		
Barometer	Maximum	30·28,	May 1st—Wind	N.
	Minimum	29·64,	Ditto 26th—Wind	N.E.

Range of the mercury . . 0·64.

	Inches.
Mean barometrical pressure for the month	30·013
——— for the lunar period ending the 7th inst. . .	29·984
——— for 17 days, with the Moon in North declin.	30·065
——— for 13 days, with the Moon in South declin.	29·902
Spaces described by the rising and falling of the mercury	3·240
Greatest variation in 24 hours	0·340
Number of changes	30·

Thermometer	Maximum	74°,	May 22d.—Wind N.
	Minimum	38	Do. 1st & 6th—Wind N.
Range		36	[& N.E.
Mean temp. of the external air		55·18	
——— for 31 days with the Sun in Taurus		51·82	
Greatest variation in 24 hours		28·00	
Mean temp. of spring water at 8 o'clock A.M. . . .		49·50	

De Luc's *Whalebone Hygrometer.**

	Degrees.
Greatest humidity of the air .	65 in the evening of the 7th.
Greatest dryness of ditto . . .	49 in the aftern. of the 8th.
Range of the index	16
Mean at 2 o'clock P.M. . . .	54·0
—— at 8 o'clock A.M. . . .	58·9
—— at 8 o'clock P.M. . . .	60·6
—— of three observations each day at 8, 2, and 8 o'clock	57·8

Evaporation for the month	4·60 inch.
Rain in the pluviameter near the ground .	2·275
Rain in ditto 23 feet high	2·115

Prevailing winds, N.E. and N.

Summary of the Weather.

A clear sky, 7; fine, with various modifications of clouds, 13; an overcast sky without rain, 7; rain, 4.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
17	9	24	0	24	20	16

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
10	10½	1	4	½	1	2	2	31

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* For 8 days only, it being under repair.

A METEOROLOGICAL TABLE: *comprising the Observations of Dr.* BURNEY *at Gosport, Mr.* J. CARY *in London, and Mr.* VEALL *at Boston.*

Gosport, at half-past Eight o'Clock, A.M.						Clouds.									Height of Barometer, in Inches, &c.		Thermometer				Rain.		Weather.		
Days of Month, 1826.	Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Evaporation.	Rain near the ground.	Lond. 1 P.M.	Bost. 8½ A.M.	London. 8 A.M.	London. Noon.	London. 11 P.M.	Boston. 8½ A.M.	London.	Boston.	London.	Boston.	Wind.
May 1	30·28	42	49·05	56	N.	1	...	1	...	1	1	...	...	......	30·24	30·06	40	52	48	47	...	...	Cloudy	Fine	NW.
2	30·17	47	......	58	N.	1	1	1	...	1	...	...	...	......	30·08	29·90	42	56	45	50	...	...	Cloudy	Fine	NW.
3	30·05	54	......	62	NE.	...	1	1	...	1	1	1	0·40	......	30·11	29·85	48	47	38	48·5	...	...	Rain	Cloudy	NW.
4	30·08	44	......	58	NE.	...	...	1	...	...	1	...	...	......	30·10	29·75	40	45	42	46	...	...	Cloudy	Rain	NW.
5	30·04	47	......	58	N.	...	...	...	...	1	1	1	...	......	30·08	29·80	45	50	39	49	...	...	Cloudy	Cloudy	NW.
6	30·05	47	......	60	N.	...	...	1	...	1	1	1	·35	0·030	30·04	29·73	43	46	39	45	...	·03	Showers	Cloudy. Rain *a.m.*	N.
7	30·05	48	49·15	61	NE.	...	1	1	...	...	1	1	...	·035	30·08	29·80	45	50	45	48	...	·05	Cloudy	Cloudy. Rain *a.m.*	N.
8	30·01	51	......	58	NE.	1	1	1	...	1	...	...	...	......	30·06	29·75	50	59	43	53·5	...	...	Fair	Fine	E.
9	30·07	50	......	...	NE.	1	...	1	...	1	...	...	·45	......	30·05	29·71	49	59	45	52	...	...	Fine	Fine	calm
10	30·10	54	......	...	SW.	1	1	1	...	1	1	1	...	......	30·08	29·65	51	61	49	55	...	...	Fair	Fine	NW.
11	30·11	56	......	...	E.	1	...	1	...	1	...	...	...	......	30·12	29·70	49	58	46	59	...	...	Cloudy	Fine	E.
12	30·22	52	......	...	NE.	1	...	...	...	1	1	...	·55	......	30·27	29·90	48	52	45	52	...	...	Cloudy	Cloudy	calm
13	30·25	49	......	...	NE.	1	...	...	...	1	...	...	...	......	30·24	30·00	47	56	40	52·5	...	...	Fine	Fine	calm
14	30·10	50	49·40	...	NE	1	...	...	...	1	...	...	...	......	30·10	29·76	46	57	41	59	...	...	Fair	Fine	E.
☽ 15	30·14	52	......	...	NE.	...	...	...	...	1	...	...	·70	......	30·16	29·72	48	56	45	55·5	...	...	Cloudy	[illegible]	calm
16	30·20	58	......	...	N.	1	1	1	...	1	1	...	...	......	30·15	29·73	48	63	56	55	...	...	Cloudy	[illegible]	W.
17	30·16	61	......	...	NW.	...	1	1	...	1	1	1	...	·050	30·15	29·70	55	64	59	60	...	...	Fair	Fine	W.
18	30·16	61	......	...	NW.	...	...	1	...	1	...	...	·70	......	30·11	29·65	60	66	58	64	...	...	Showers	Fine	W.
19	29·97	58	......	...	S.	1	...	1	...	...	1	1	...	·265	29·87	29·35	55	65	56	66	...	...	Fair	Fine	calm
20	29·70	57	49·55	...	SE.	...	...	1	...	1	1	1	...	·270	29·85	29·32	54	62	46	58·5	...	...	Fair	Fine	calm
○ 21	30·04	55	......	...	N.	1	...	...	...	1	...	...	·40	......	30·08	29·72	52	63	54	56	...	...	Fine	Cloudy	calm
22	30·10	61	......	...	N.	...	...	1	...	1	...	...	...	......	30·00	29·70	59	70	52	63·5	...	...	Fine	Fine	NE.
23	30·05	61	......	...	N.	1	...	1	...	1	1	1	...	·005	29·95	29·70	58	59	50	60	...	...	Cloudy	Fine	NE.
24	29·88	61	......	...	N.	1	1	1	...	1	1	1	·45	·175	29·84	29·53	54	64	53	59	...	·04	Rain	Cloudy. Rain *a.m.*	calm
25	29·73	56	......	...	N.	...	...	1	...	...	1	1	...	·115	29·70	29·45	52	56	52	59	...	...	Rain	Cloudy. Rain *p.m.*	E.
26	29·64	60	49·90	...	NE.	1	...	1	...	1	1	1	...	·035	29·77	29·40	55	62	55	56·5	...	·19	Showers	Cloudy	E.
27	29·80	64	......	...	SE.	1	1	1	...	1	1	...	·30	......	29·83	29·54	56	63	50	60	...	...	Cloudy	Cloudy	E.
☾ 28	29·90	63	......	...	NE.	1	...	1	...	1	1	1	...	·760	29·90	29·63	55	60	51	55	...	...	Cloudy	Cloudy	NE.
29	29·79	53	......	...	N.	...	...	1	...	...	...	1	...	·365	29·76	29·53	50	54	51	52·5	...	...	Rain	Cloudy. Rain at	NE.
30	29·90	54	......	...	N.	...	...	...	...	...	1	1	...	·025	29·96	29·67	52	55	51	52·5	...	·13	Rain	Cloudy [night.	NE.
31	29·92	59	49·95	...	N.	...	...	1	...	...	1	1	·30	·145	29·94	29·66	53	57	53	54	...	...	Cloudy	Cloudy	NE.
Average.	30·021	54·35	49·50	58·9		17	9	24		24	20	16	4·60	2·275	30·18	29·68	50	58	48	54·9	2·30	0·44			

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